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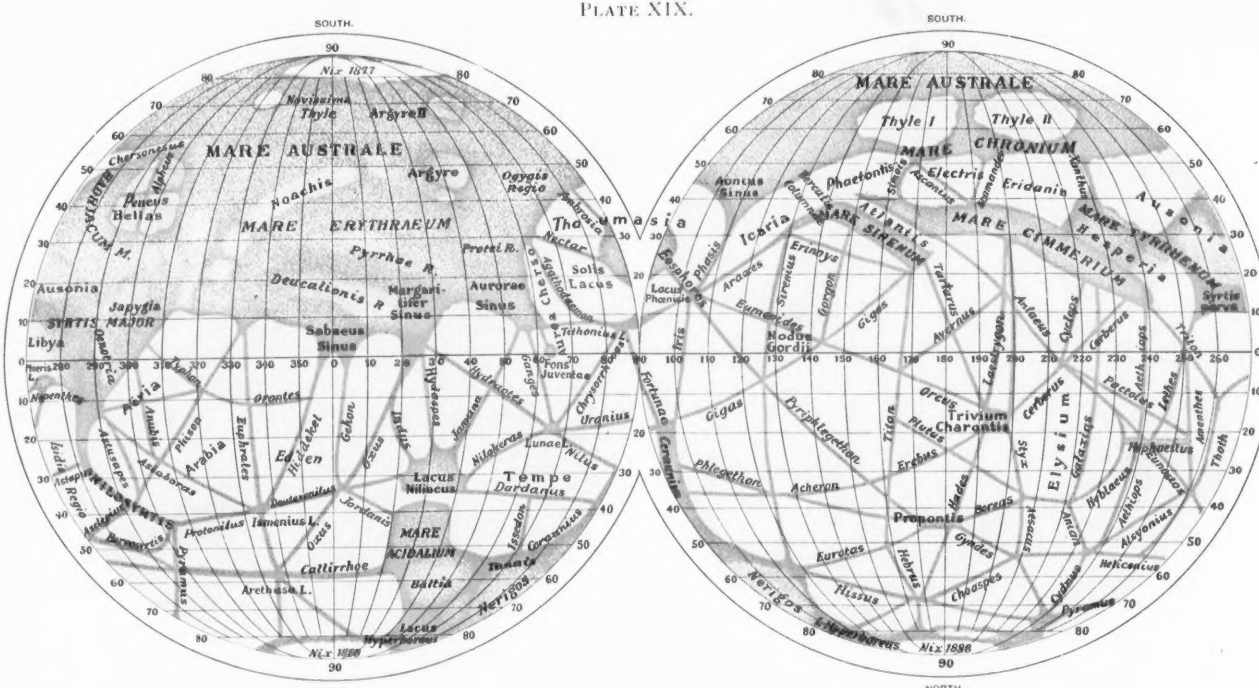
PLATE XIX.



SCHIAPARELLI'S MAP OF MARS, 1888,

With a few names added from his earlier charts.

Astronomy and Astro-Physics.

VOL. XIII, No. 8. OCTOBER, 1894. WHOLE No. 128.

General Astronomy.

ON THE MAGNITUDE OF THE SOLAR SYSTEM.*

WM. HARKNESS.

Nature may be studied in two widely different ways. On the one hand we may employ a powerful microscope which will render visible the minutest forms and limit our field of view to an infinitesimal fraction of an inch situated within a foot of our own noses; or on the other hand, we may occupy some commanding position, and from thence, aided perhaps by a telescope, we may obtain a comprehensive view of an extensive region. The first method is that of the specialist, the second is that of the philosopher, but both are necessary for an adequate understanding of nature. The one has brought us knowledge wherewith to defend ourselves against bacteria and microbes which are among the most deadly enemies of mankind, and the other has made us acquainted with the great laws of matter and force upon which rests the whole fabric of science. All nature is one, but for convenience of classification we have divided our knowledge into a number of sciences which we usually regard as quite distinct from each other. Along certain lines, or more properly, in certain regions, these sciences necessarily abut on each other, and just there lies the weakness of the specialist. He is like a wayfarer who always finds obstacles in crossing the boundaries between two countries, while to the traveler who gazes over them from a commanding eminence the case is quite different. If the boundary is an ocean shore there is no mistaking it; if a broad river or a chain of mountains it is still distinct; but if only a line of posts traced over hill and dale, then it becomes lost in the natural features of the landscape, and the essential unity of the whole region is apparent. In that case the border land is wholly a human conception of which nature takes no cognizance, and so it is with the scientific border land to which I propose to invite your attention this evening.

* Communicated by the author; being the presidential address delivered before the American Association for the advancement of Science, at its Brooklyn meeting, August 16, 1894.

To the popular mind there are no two sciences further apart than astronomy and geology. The one treats of the structure and mineral constitution of our earth, the causes of its physical features and its history, while the other treats of the celestial bodies, their magnitudes, motions, distances, periods of revolution, eclipses, order, and of the causes of their various phenomena. And yet many, perhaps I may even say most, of the apparent motions of the heavenly bodies are merely reflections of the motions of the Earth, and in studying them we are really studying it. Furthermore, precession, nutation and the phenomena of the tides depend largely upon the internal structure of the Earth, and there astronomy and geology merge into each other. Nevertheless, the methods of the two sciences are widely different, most astronomical problems being discussed quantitatively by means of rigid mathematical formulæ, while in the vast majority of cases the geological ones are discussed only qualitatively, each author contenting himself with a mere statement of what he thinks. With precise data the methods of astronomy lead to very exact results, for mathematics is a mill which grinds exceeding fine; but after all, what comes out of a mill depends wholly upon what is put into it, and if the data are uncertain, as is the case in most cosmological problems, there is little to choose between the mathematics of the astronomer and the guesses of the geologist.

If we examine the addresses delivered by former presidents of this Association, and of the sister—perhaps it would be nearer the truth to say the parent Association, on the other side of the Atlantic, we shall find that they have generally dealt either with the recent advances in some broad field of science, or else with the development of some special subject. This evening I propose to adopt the latter course, and I shall invite your attention to the present condition of our knowledge respecting the magnitude of the solar system, but in so doing it will be necessary to introduce some considerations derived from laboratory experiments upon the luminiferous ether, others derived from experiments upon ponderable matter, and still others relating both to the surface phenomena and to the internal structure of the earth, and thus we shall deal largely with the border land where astronomy, physics and geology merge into each other.

The relative distances of the various bodies which compose the solar system can be determined to a considerable degree of approximation with very crude instruments as soon as the true plan of the system becomes known, and that plan was taught by

Pythagoras more than five hundred years before Christ. It must have been known to the Egyptians and Chaldeans still earlier, if Pythagoras really acquired his knowledge of astronomy from them, as is affirmed by some of the ancient writers, but on that point there is no certainty. In public Pythagoras seemingly accepted the current belief of his time, which made the Earth the center of the universe, but to his own chosen disciples he communicated the true doctrine that the Sun occupies the center of the solar system and that the Earth is only one of the planets revolving around it. Like all the world's greatest sages he seems to have taught only orally. A century elapsed before his doctrines were reduced to writing by Philolaus of Crotona, and it was still later before they were taught in public for the first time by Hicetas, or as he is sometimes called Nicetas, of Syracuse. Then the familiar cry of impiety was raised and the Pythagorean system was eventually suppressed by that now called the Ptolemaic, which held the field until it was overthrown by Copernicus almost two thousand years later. Pliny tells us that Pythagoras believed the distances to the Sun and Moon to be respectively 252,000 and 12,600 stadia, or, taking the stadium at 625 feet, 29,837 and 1,492 English miles; but there is no record of the method by which these numbers were ascertained.

After the relative distances of the various planets are known, it only remains to determine the scale of the system, for which purpose the distance between any two planets suffices. We know little about the early history of the subject, but it is clear that the primitive astronomers must have found the quantities to be measured too small for detection with their instruments, and even in modern times the problem has proved to be an extremely difficult one. Aristarchus of Samos, who flourished about 270 B. C., seems to have been the first to attack it in a scientific manner. Stated in modern language his reasoning was that when the Moon is exactly half full the Earth and Sun, as seen from its centre, must make a right angle with each other, and by measuring the angle between the Sun and Moon, as seen from the Earth at that instant, all the angles of the triangle joining the Earth, Sun and Moon would become known, and thus the ratio of the distance of the Sun to the distance of the Moon would be determined. Although perfectly correct in theory, the difficulty of deciding visually upon the exact instant when the Moon is half full is so great that it cannot be accurately done, even with the most powerful telescopes. Of course Aristarchus had no telescope, and he does not explain how he effected the observation, but his con-

clusion was that at the instant in question the distance between the centers of the Sun and Moon as seen from the Earth, is less than a right angle by 1-30th part of the same. We should now express this by saying that the angle is 87 degrees, but Aristarchus knew nothing of trigonometry, and in order to solve his triangle he had recourse to an ingenious, but long and cumbersome geometrical process which has come down to us and affords conclusive proof of the condition of Greek mathematics at that time. His conclusion was that the Sun is nineteen times further from the Earth than the Moon, and if we combine that result with the modern value of the Moon's parallax, viz., 3,422.38 seconds, we obtain for the solar parallax 180 seconds, which is more than twenty times too great.

The only other method of determining the solar parallax known to the ancients was that devised by Hipparchus about 150 B. C. It was based on measuring the rate of decrease of the diameter of the Earth's shadow cone by noting the duration of lunar eclipses, and as the result deduced from it happened to be nearly the same as that found by Aristarchus, substantially his value of the parallax remained in vogue for nearly two thousand years, and the discovery of the telescope was required to reveal its erroneous character. Doubtless this persistency was due to the extreme minuteness of the true parallax, which we now know is far too small to have been visible upon the ancient instruments, and thus the supposed measures of it were really nothing but measures of their inaccuracy.

The telescope was first pointed to the heavens by Galileo in 1609, but it needed a micrometer to convert it into an accurate measuring instrument, and that did not come into being until 1639, when it was invented by William Gascoigne. After his death, in 1644, his original instrument passed to Richard Townley, who attached it to a fourteen-foot telescope at his residence in Townley, Lancashire, England, where it was used by Flamsteed in observing the diurnal parallax of Mars during its opposition in 1672. A description of Gascoigne's micrometer was published in the Philosophical Transactions in 1667, and a little before that a similar instrument had been invented by Auzout, in France, but observatories were fewer then than now, and so far as I know J. D. Cassini was the only person beside Flamsteed who attempted to determine the solar parallax from that opposition of Mars. Foreseeing the importance of the opportunity, he had Richer dispatched to Cayenne some months previously, and when the opposition came he effected two determinations of the

parallax; one being by the diurnal method, from his own observations in Paris, and the other by the meridian method, from observations in France by himself, Römer and Picard, combined with those of Richer at Cayenne. This was the transition from the ancient instruments with open sights to telescopes armed with micrometers, and the result must have been little short of stunning to the seventeenth century astronomers, for it caused the hoary and gigantic parallax of about 180 seconds to shrink incontinently to 10 seconds, and thus expanded their conception of the solar system to something like its true dimensions. More than fifty years previously Kepler had argued from his ideas of the celestial harmonies that the solar parallax could not exceed 60 seconds, and a little later Horrocks had shown on more scientific grounds that it was probably as small as 14 seconds, but the final death blow to the ancient values ranging as high as two or three minutes came from these observations of Mars by Flamsteed, Cassini and Richer.

Of course the results obtained in 1672 produced a keen desire on the part of astronomers for further evidence respecting the true value of the parallax, and as Mars comes into a favorable position for such investigations only at intervals of about sixteen years, they had recourse to observations of Mercury and Venus. In 1677 Halley observed the diurnal parallax of Mercury, and also a transit of that planet across the Sun's disk, at St. Helena, and in 1681 J. D. Cassini and Picard observed Venus when she was on the same parallel with the Sun, but although the observations of Venus gave better results than those of Mercury, neither of them was conclusive, and we now know that such methods are inaccurate even with the powerful instruments of the present day. Nevertheless Halley's attempt by means of the transit of Mercury ultimately bore fruit in the shape of his celebrated paper of 1716, wherein he showed the peculiar advantages of transits of Venus for determining the solar parallax. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks, but Halley was the first to work it out completely, and long after his death his paper was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus in 1761 and 1769, from which our first accurate knowledge of the Sun's distance was obtained.

Those who are not familiar with practical astronomy may wonder why the solar parallax can be got from Mars and Venus, but not from Mercury or the Sun itself. The explanation de-

pends upon two facts. Firstly, the nearest approach of these bodies to the Earth is for Mars 33,874,000 miles, for Venus 23,-654,000 miles, for Mercury 47,935,000 miles, and for the Sun 91,239,000 miles. Consequently, for us Mars and Venus have very much larger parallaxes than Mercury or the Sun, and of course the larger the parallax the easier it is to measure. Secondly even the largest of these parallaxes must be determined within far less than one-tenth of a second of the truth, and while that degree of accuracy is possible in measuring short arcs, it is quite unattainable in long ones. Hence one of the most essential conditions for the successful measurement of parallaxes is that we shall be able to compare the place of the near body with that of a more distant one situated in the same region of the sky. In the case of Mars that can always be done by making use of a neighboring star, but when Venus is near the Earth she is also so close to the Sun that stars are not available, and consequently her parallax can be satisfactorily measured only when her position can be accurately referred to that of the Sun, or in other words, only during her transits across the Sun's disc. But even when the two bodies to be compared are sufficiently near each other, we are still embarrassed by the fact that it is more difficult to measure the distance between the limb of a planet and a star or the limb of the Sun than it is to measure the distance between two stars, and since the discovery of so many asteroids that circumstance has led to their use for the determination of the solar parallax. Some of these bodies approach within 75,230,000 miles of the Earth's orbit, and as they look precisely like stars, the increased accuracy of pointing on them fully makes up for their greater distance as compared with Mars or Venus.

After the Copernican system of the world and the Newtonian theory of gravitation were accepted it soon became evident that trigonometrical measurements of the solar parallax might be supplemented by determinations based on the theory of gravitation, and the first attempts in that direction were made by Machin in 1729 and T. Mayer in 1753. The measurement of the velocity of light between points on the Earth's surface, first effected by Fizeau in 1849, opened up still other possibilites, and thus for determining the solar parallax we have at our command no less than three entirely distinct classes of methods, which are known respectively as the trigonometrical, the gravitational and the photo-tachymetrical. We have already given a summary sketch of the trigonometrical methods as applied by the ancient astronomers to the dichotomy and shadow cone of the Moon,

and by the moderns to Venus, Mars and the asteroids, and we shall next glance briefly at the gravitational and photo-tachymetrical methods.

The gravitational results which enter directly or indirectly into the solar parallax are six in number, to wit: first, the relation of the Moon's mass to the tides; second, the relation of the Moon's mass and parallax to the force of gravity at the Earth's surface; third, the relation of the solar parallax to the masses of the Earth and Moon; fourth, the relation of the solar and lunar parallaxes to the Moon's mass and parallactic inequality; fifth, the relation of the solar and lunar parallaxes to the Moon's mass and the Earth's lunar inequality; sixth, the relation of the constants of nutation and precession to the Moon's parallax.

Respecting the first of these relations it is to be remarked that the tide producing forces are the attractions of the Sun and Moon upon the waters of the ocean, and from the ratio of these attractions the Moon's mass can readily be determined. But unfortunately the ratio of the solar tides to the lunar tides is affected both by the depth of the sea and by the character of the channels through which the water flows, and for that reason the observed ratio of these tides requires multiplication by a correcting factor in order to convert it into the ratio of the forces. The matter is further complicated by this correcting factor varying from port to port, and in order to get satisfactory results long series of observations are necessary. The labor of deriving the Moon's mass in this way was formerly so great that for more than half a century La Place's determination from the tides at Brest remained unique, but the recent application of harmonic analysis to the data supplied by self registering tide gauges is likely to yield abundant results in the near future.

Our second gravitational relation, viz., that connecting the Moon's mass and parallax with the force of gravity at the Earth's surface, affords an indirect method of determining the Moon's parallax with very great accuracy if the computation is carefully made, and with a fair approximation to the truth even when the data are exceedingly crude. To illustrate this, let us see what could be done with a railroad transit such as is commonly used by surveyors, a steel tape, and a fairly good watch. Neglecting small corrections due to the flattening of the Earth, the centrifugal force at its surface, the eccentricity of its orbit and the mass of the Moon; the law of gravitation shows that if we multiply together the length of the seconds pendulum, the square of the radius of the Earth and the square of the length of

the sidereal month, divide the product by four, and take the cube root of the quotient, the result will be the distance from the Earth to the Moon. To find the length of the seconds pendulum we would rate the watch by means of the railroad transit, and then making a pendulum out of a spherical leaden bullet suspended by a fine thread, we would adjust the length of the thread until the pendulum made exactly 300 vibrations in five minutes by the watch. Then, supposing the experiment to be made here or in New York City, we would find that the distance from the point of suspension of the thread to the centre of the bullet was about 39⅛ inches, and dividing that by the number of inches in a mile, viz., 63,360, we would have for the length of the seconds pendulum one sixteen hundred and twentieth of a mile. The next step would be to ascertain the radius of the Earth, and the quickest way of doing so would probably be, first, to determine the latitude of some point in New York City by means of the railroad transit; next to run a traverse survey along the old post road from New York to Albany, and finally to determine the latitude of some point in Albany. The traverse survey should surely be correct to one part in three hundred, and as the distance between the two cities is about two degrees, the difference of latitude might be determined to about the same percentage of accuracy. In that way we would find the length of two degrees of latitude to be about 138 miles, whence the Earth's radius would be 3,953 miles. It would then only remain to observe the time occupied by the Moon in making a sidereal revolution around the Earth, or, in other words, the time which she occupies in moving from any given star back to the same star again. By noting that to within one quarter of her own diameter we would soon find that the time of revolution is about 27.32 days, and multiplying that by the number of seconds in a day, viz., 86,400, we would have for the length of the sidereal month 2,360,000 seconds. With these data the computation would stand as follows: The radius of the Earth, 3,953 miles, multiplied by the length of a sidereal month, 2,360,000 seconds, and the product squared gives 87,060,000,000,000,000,000. Multiplying that by one-fourth of the length of the seconds pendulum, viz., $\frac{1}{6480}$ of a mile, and extracting the cube root of the product, we would get 237,700 miles for the distance from the Earth to the Moon, which is only about 850 miles less than the truth, and certainly a remarkable result considering the crudeness of the instruments by which it might be obtained. Nevertheless, when all the conditions are rigorously taken into account these

data are to be regarded as determining the relation between the Moon's mass and parallax, rather than the parallax itself.

Our third gravitational relation, to wit: that existing between the solar parallax, the solar attractive force and the masses of the Earth and Moon, is analagous to the relation existing between the Moon's mass and parallax and the force of gravity at the Earth's surface, but it cannot be applied in exactly the same way on account of our inability to swing a pendulum on the Sun. We are therefore compelled to adopt some other method of determining the Sun's attractive force, and the most available is that which consists in observing the perturbative action of the Earth and Moon upon our nearest planetary neighbors, Venus and Mars. From this action the law of gravitation enables us to determine the ratio of the Sun's mass to the combined masses of the Earth and Moon, and then the relation in question furnishes a means of comparing the masses so found with trigonometrical determinations of the solar parallax. Thus it appears that notwithstanding necessary differences in the methods of procedure, the analogy between the second and third gravitational relations holds not only with respect to their theoretical basis, but also in their practical application, the one being used to determine the relation between the mass of the Moon and its distance from the Earth, and the other to determine the relation between the combined masses of the Earth and Moon and their distance from the Sun.

Our fourth gravitational relation deals with the connection between the solar parallax, the lunar parallax, the Moon's mass and the Moon's parallactic inequality. The important quantities are here the solar parallax and the Moon's parallactic inequality, and although the derivation of the complete expression for the connection between them is a little complicated, there is no difficulty in getting a general notion of the forces involved. As the Moon moves around the Earth she is alternately without and within the Earth's orbit. When she is without, the Sun's attraction on her acts with that of the Earth; when she is within, the two attractions act in opposite directions. Thus in effect the centripetal force holding the Moon to the Earth is alternately increased and diminished, with the result of elongating the Moon's orbit toward the Sun and compressing it on the opposite side. As the variation of the centripetal force is not great, the change of form of the orbit is small; nevertheless, the summation of the minute alterations thereby produced in the Moon's orbital velocity suffices to put her sometimes ahead and sometimes behind

her mean place to an extent which oscillates from a maximum to a minimum, as the Earth passes from perihelion to aphelion, and averages about 125 seconds of arc. This perturbation of the Moon is known as the parallactic inequality, because it depends on the Earth's distance from the Sun, and can therefore be expressed in terms of the solar parallax. Conversely, the solar parallax can be deduced from the observed value of the parallatic inequality, but unfortunately there are great practical difficulties in making the requisite observations with a sufficient degree of accuracy. Notwithstanding the ever recurring talk about the advantages to be obtained by observing a small, well-defined crater instead of the Moon's limb, astronomers have hitherto found it impracticable to use anything but the limb, and the disadvantage of doing so, as compared with observing a star, is still further increased by the circumstance that in general only one limb can be seen at a time, the other being shrouded in darkness. If both limbs could always be observed we should then have a uniform system of data for determining the place of the centre, but under existing circumstances we are compelled to make our observations half upon one limb and half upon the other, and thus they involve all the systematic errors which may arise from the conditions under which these limbs are observed, and all the uncertainty which attaches to irradiation, personal equation and our defective knowledge of the Moon's semi-diameter.

Our fifth gravitational relation is that which exists between the solar parallax, the lunar parallax, the Moon's mass and the Earth's lunar inequality. Strictly speaking the Moon does not revolve around the Earth's center, but both bodies revolve around the common center of gravity of the two. In consequence of that an irregularity arises in the Earth's orbital velocity around the Sun, the common center of gravity moving in accordance with the laws of elliptic motion, while the Earth, on account of its revolution around that center, undergoes an alternate acceleration and retardation which has for its period a lunar month, and is called the lunar inequality of the Earth's motion. We perceive this inequality as an oscillation superposed on the elliptic motion of the Sun, and its semi-amplitude is the measure of the angle subtended at the Sun by the interval between the center of the Earth and the common center of gravity of the Earth and Moon. Just as an astronomer on the Moon might use the radius of her orbit around the Earth as a base for measuring her distance from the Sun, so we may use this interval for the same purpose. We find

its length in miles from the equatorial semi-diameter of the Earth, the Moon's parallax and the Moon's mass, and thus we have all the data for determining the solar parallax from the inequality in question. In view of the great difficulty which has been experienced in measuring the solar parallax itself, it may be asked why we should attempt to deal with the parallactic inequality, which is about 26 per cent smaller? The answer is, because the latter is derived from differences of the Sun's right ascension, which are furnished by the principal observatories in vast numbers, and should give very accurate results on account of their being made by methods which insure freedom from constant errors. Nevertheless, the Sun is not so well adapted for precise observations as the stars, and Dr. Gill has recently found that heliometer measurements upon asteroids which approach very near to the Earth yield values of the parallactic inequality superior to those obtained from right ascensions of the Sun.

Our sixth gravitational relation is that which exists between the Moon's parallax and the constants of precession and nutation. Every particle of the Earth is attracted both by the Sun and by the Moon, but in consequence of the polar flattening the resultant of these attractions passes a little to one side of the Earth's center of gravity. Thus a couple is set up, which, by its action upon the rotating Earth, causes the axis thereof to describe a surface which may be called a fluted cone, with its apex at the Earth's center. A top spinning with its axis inclined describes a similar cone, except that the flutings are absent and the apex is at the point upon which the spinning occurs. For convenience of computation we resolve this action into two components, and we name that which produces the cone the luni-solar precession, and that which produces the flutings the nutation. In this phenomenon the part played by the Sun is comparatively small, and by eliminating it we obtain a relation between the luni-solar precession, the nutation and the Moon's parallax which can be used to verify and correct the observed values of these quantities.

In the preceding paragraph we have seen that the relation between the quantities there considered depends largely upon the flattening of the Earth, and thus we are lead to inquire how and with what degree of accuracy that is determined. There are five methods, viz.: one geodetic, one gravitational, and three astronomical. The geodetic method depends upon measurements of the length of a degree on various parts of the Earth's surface; and with the data hitherto accumulated it has proved quite un-

satisfactory. The gravitational method consists in determining the length of the seconds pendulum over as great a range of latitude as possible, and deducing therefrom the ratio of the Earth's polar and equatorial semi-diameters by means of Clairaut's theorem. The pendulum experiments show that the Earth's crust is less dense on mountain plateaux than at the sea coast, and thus for the first time we are brought into contact with geological considerations. The first astronomical method consists in observing the Moon's parallax from various points on the Earth's surface, and as these parallaxes are nothing else than the angular semi-diameter of the Earth at the respective points, as seen from the Moon, they afford a direct measure of the flattening. The second and third astronomical methods are based upon certain perturbations of the Moon which depend upon the figure of the Earth, and should give extremely accurate results, but unfortunately very great difficulties oppose themselves to the exact measurement of the perturbations. There is also an astronomico-geological method which cannot yet be regarded as conclusive on account of our lack of knowledge respecting the law of density which prevails in the interior of the Earth. It is based upon the fact that a certain function of the Earth's moments of inertia can be determined from the observed values of the coefficients of precession and nutation, and could also be determined from the figure and dimensions of the Earth if we knew the exact distribution of matter in its interior. Our present knowledge on that subject is limited to a superficial layer not more than ten miles thick, but it is usual to assume that the deeper matter is distributed, according to La Grange's law, and then by writing the function in question in a form which leaves the flattening indeterminate, and equating the expression so found to the value given by the precession and nutation, we readily obtain the flattening. As yet these methods do not give consistent results, and so long as serious discrepancies remain between them there can be no security that we have arrived at the truth.

It should be remarked that in order to compute the function of the Earth's moments of inertia which we have just been considering, we require not only the figure and dimensions of the Earth and the law of distribution of density in its interior, but also its mean and surface densities. The experiments for determining the mean density have consisted in comparing the Earth's attraction with the attraction either of a mountain, or of a known thickness of the Earth's crust, or of a known mass of metal. In the case of mountains the comparisons have been made with

plumb lines and pendulums; in the case of known layers of the Earth's crust they have been made by swinging pendulums at the surface and down in mines; and in the case of known masses of metal they have been made with torsion balances, fine chemical balances and pendulums. The surface density results from a study of the materials composing the Earth's crust, but notwithstanding the apparent simplicity of that process, it is doubtful if we have yet attained as accurate a result as in the case of the mean density.

Before quitting this part of our subject, it is important to point out that the luni-solar precession cannot be directly observed, but must be derived from the general precession. The former of these qualities depends only upon the action of the Sun and Moon, while the latter is affected in addition by the action of all the planets, and to ascertain what that is we must determine their masses. The methods of doing so fall into two great classes, according as the planets dealt with have or have not satellites. The most favorable case is that in which one or more satellites are present, because the mass of the primary follows immediately from their distances and revolution times, but even then there is a difficulty in the way of obtaining very exact results. By extending the observations over sufficiently long periods the revolution times may be ascertained with any desired degree of accuracy, but all measurements of the distance of a satellite from its primary are affected by personal equation, which we cannot be sure of completely eliminating, and thus a considerable margin of uncertainty is brought into the masses. In the cases of Mercury and Venus, which have no satellites, and to a certain extent in the case of the Earth also, the only available way of ascertaining the masses is from the pertnrbations produced by the action of the various planets on each other. These perturbations are of two kinds, periodic and secular. When sufficient data have been accumulated for the exact determination of the secular perturbations, they will give the best results, but as yet it remains advantageous to employ the periodic perturbations also.

Passing now to the photo-tachymetrical methods, we have first to glance briefly at the mechanical appliances by which the tremendous velocity of light has been successfully measured. They are of the simplest possible character, and are based either upon a toothed wheel, or upon a revolving mirror.

The toothed-wheel method was first used by Fizeau in 1849. To understand its operation, imagine a gun barrel with a toothed wheel revolving at right angles to its muzzle in such a way that

the barrel is alternately closed and opened as the teeth and the spaces between them pass before it. Then, with the wheel in rapid motion, at the instant when a space is opposite the muzzle let a ball be fired. It will pass out freely, and after traversing a certain distance, let it strike an elastic cushion and be reflected back upon its own path. When it reaches the wheel, if it hits a space it will return into the gun barrel, but if it hits a tooth it will be stopped. Examining the matter a little more closely we see that as the ball requires a certain time to go and return, if, during that time the wheel moves through an odd multiple of the angle between a space and a tooth the ball will be stopped, while if it moves through an even multiple of that angle the ball will return into the barrel. Now imagine the gun barrel, the ball and the elastic cushion to be replaced respectively by a telescope, a light wave and a mirror. Then if the wheel moved at such a speed that the returning light wave struck against the tooth following the space through which it issued, to an eye looking into the telescope all would be darkness. If the wheel moved a little faster and the returning light wave passed through the space succeeding that through which it issued, the eye at the telescope would perceive a flash of light, and if the speed was continuously increased a continual succession of eclipses and illuminations would follow each other according as the returning light was stopped against a tooth or passed through a space further and further behind that through which it issued. Under these conditions the time occupied by the light in traversing the space from the wheel to the mirror and back again would evidently be the same as the time required by the wheel to revolve through the angle between the space through which the light issued and that through which it returned, and thus the velocity of light would become known from the distance between the telescope and the mirror, together with the speed of the wheel. Of course the longer the distance traversed and the greater the velocity of the wheel the more accurate would be the result.

The revolving mirror method was first used by Foucault in 1862. Conceive the toothed wheel of Fizeau's apparatus to be replaced by a mirror attached to a vertical axis and capable of being put into rapid rotation. Then it will be possible so to arrange the apparatus that light issuing from the telescope shall strike the movable mirror and be reflected to the distant mirror, whence it will be returned to the movable mirror again, and being thrown back into the telescope will appear as a star in the center of the field of view. That adjustment being made, if the

mirror were caused to revolve at a speed of some hundred turns per second it would move through an appreciable angle while the light was passing from it to the distant mirror and back again, and in accordance with the laws of reflection, the star in the field of the telescope would move from the center by twice the angle through which the mirror had turned. Thus the deviation of the star from the center of the field would measure the angle through which the mirror turned during the time occupied by light in passing twice over the interval between the fixed and revolving mirrors, and from the magnitude of that angle, together with the known speed of the mirror, the velocity of the light could be calculated.

In applying either of these methods the resulting velocity is that of light when traversing the Earth's atmosphere, but what we want is its velocity in space, which we suppose to be destitute of ponderable material, and in order to obtain that the velocity in the atmosphere must be multiplied by the refractive index of air. The correct velocity so obtained can then be used to find the solar parallax, either from the time required by light to traverse the semi-diameter of the Earth's orbit, or from the ratio of the velocity of light to the orbital velocity of the Earth.

Any periodic correction which occurs in computing the place of a heavenly body, or the time of a celestial phenomenon, is called by astronomers an equation, and as the time required by light to traverse the semi-diameter of the Earth's orbit first presented itself in the guise of a correction to the computed times of the eclipses of Jupiter's satellites, it has received the name of the light equation. The Earth's orbit being interior to that of Jupiter, and both having the Sun for their centre, it is evident that the distances between the two planets must vary from the sum to the difference of the radii of their respective orbits, and the time required by light to travel from one planet to the other must vary proportionately. Consequently, if the observed times of the eclipses of Jupiter's satellites are compared with the times computed upon the asumption that the two planets are always separated by their mean distance, it will be found that the eclipses occur too early when the Earth is at less than its mean distance from Jupiter, and too late when it is farther off, and from large numbers of such observations the value of the light equation has been deduced.

The combination of the motion of light through our atmosphere with the orbital motion of the Earth gives rise to the annual aberration, all the phases of which are computed from its

maximum value, commonly called the constant of aberration. There is also a diurnal aberration due to the rotation of the Earth on its axis, but that is quite small and does not concern us this evening. When aberration was discovered the corpuscular theory of light was in vogue, and it offered a charmingly simple explanation of the whole phenomenon. The hypothetical light corpuscles impinging upon the Earth were thought to behave precisely like the drops in a shower of rain, and you all know that their apparent direction is affected by any motion on the part of the observer. In a calm day, when the drops are falling perpendicularly, a man standing still holds his umbrella directly over his head, but as soon as he begins to move forward he inclines his umbrella in the same direction, and the more rapidly he moves the greater must be its inclination in order to meet the descending shower. Similarly, the apparent direction of on coming light corpuscles would be affected by the orbital motion of the Earth, so that in effect it would always be the resultant arising from combining the motion of the light with a motion equal and opposite to that of the Earth. But since the falsity of the corpuscular theory has been proved that explanation is no longer tenable, and as yet we have not been able to replace it with anything equally satisfactory based on the now universally accepted undulatory theory. In accordance with the latter theory we must conceive the Earth as plowing its way through the ether, and the point which has hitherto baffled us is whether or not in so doing it produces any disturbance of the ether which affects the aberration. In our present ignorance on that point we can only say that the aberration constant is certainly very nearly equal to the ratio of the Earth's orbital velocity to the velocity of light, but we cannot affirm that it is rigorously so.

The luminiferous ether was invented to account for the phenomena of light, and for two hundred years it was not suspected of having any other function. The emission theory postulated only the corpuscles which constitute light itself, but the undulatory theory fills all space with an imponderable substance possessing properties even more remarkable than those of ordinary matter, and to some of the acutest intellects the magnitude of this idea has proved an almost insuperable objection against the whole theory. So late as 1862 Sir David Brewster, who had gained a world wide reputation by his optical researches, expressed himself as staggered by the notion of filling all space with some substance merely to enable a little twinkling star to send its light to us; but not long after Clerk Maxwell removed that

difficulty by a discovery coextensive with the undulatory theory itself. Since 1845, when Faraday first performed his celebrated experiment of magnetizing a ray of light, the idea that electricity is a phenomenon of the ether had been steadily growing, until at last Maxwell perceived that if such were the fact the rate of propagation of an electro-magnetic wave must be the same as the velocity of light. At that time no one knew how to generate such waves, but Maxwell's theory showed him that their velocity must be equal to the number of electric units of quantity in the electro-magnet unit, and careful experiments soon proved that that is the velocity of light. Thus it was put almost beyond the possibility of doubt that the ether gives rise to the phenomena of electricity and magnetism as well as to those of light, and perhaps it may even be concerned in the production of gravitation itself. What could be apparently more remote that these electric quantities and the solar parallax? And yet we have here a relation between them, but we make no use of it because as yet the same relation can be far more accurately determined from experiments upon the velocity of light.

Now let us recall the quantities and methods of observation which we have found to be involved, either directly or indirectly, with the solar parallax. They are, the solar parallax, obtained from transits of Venus, oppositions of Mars and oppositions of certain asteroids; the lunar parallax, found both directly and from measurements of the force of gravity at the Earth's surface; the constants of precession, nutation and aberration, obtained from observations of the stars; the parallactic inequality of the Moon; the lunar inequality of the Earth, usually obtained from observations of the Sun, but recently found from heliometer observations of certain asteroids; the mass of the Earth, found from the solar parallax, and also from the periodic and secular perturbations of Venus and Mars; the mass of the Moon, found from the lunar inequality of the Earth, and also from the ratio of the solar and lunar components of the ocean tides; the masses of all the planets, obtained from observations of their satellites whenever possible, and when no satellites exist, then from observations of their mutual perturbations, both periodic and secular; the velocity of light, obtained from experiments with revolving mirrors and toothed wheels, together with laboratory determinations of the index of refraction of atmospheric air; the light equation, obtained from observations of the eclipses of Jupiter's satellites; the figure of the Earth, obtained from geodetic triangulations, measurements of the length of the seconds pendulum

in various latitudes, and observations of certain perturbations of the Moon; the mean density of the Earth, obtained from measurements of the attractions of mountains, from pendulum experiments in mines, and from experiments on the attraction of known masses of matter made either with torsion balances or with the most delicate chemical balances; the surface density of the Earth, obtained from geological examinations of the surface strata;, and lastly, the law of distribution of density in the interior of the Earth, which in the present state of geological knowledge we can do little more than guess at.

Here then we have a large group of astronomical, geodetic, geological and physical quantities which must all be considered in finding the solar parallax, and which are all so entangled with each other that no one of them can be varied without affecting all the rest. It is therefore impossible to make an accurate determination of any one of them apart from the remainder of the group, and thus we are driven to the conclusion that they must all be determined simultaneously. Such has not been the practice of astronomers in the past, but it is the method to which they must inevitably resort in the future. A cursory glance at an analogous problem occuring in geodesy may be instructive. When a country is covered with a net of triangles it is always found that the observed angles are subject to a certain amount of error, and a century ago it was the habit to correct the angles in each triangle without much regard to the effect upon adjacent triangles. Consequently the adjustment of the errors was imperfect, and in computing the interval between any two distant points the result would vary somewhat with the triangles used in the computation—that is, if one computation was made through a chain of triangles running around on the right hand side, another through a chain of triangles running straight between the two points, and a third through a chain of triangles running around on the left hand side, the results were usually all different. At that time things were less highly specialized than now, and all geodetic operations were yet in the hands of first rate astronomers, who soon devised processes for overcoming the difficulty. They imagined every observed angle to be subject to a small correction, and as these corrections were all entangled with each other through the geometrical conditions of the net, by a most ingenious application of the method of least squares they determined them all simultaneously in such a way as to satisfy the whole of the geometrical conditions. Thus the best possible adjustment was obtained, and no matter what triangles were used in passing

from one point to another, the result was always the same. That method is now applied to every important triangulation, and its omission would be regarded as proof of incompetency on the part of those in charge of the work.

Now let us compare the conditions existing respectively in a triangulation net and in the group of quantities for the determination of the solar parallax. In the net every angle is subject to a small correction, and the whole system of corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy all the geometrical conditions of the net. Like the triangles, the quantities composing the group from which the solar parallax must be determined are all subject to error, and therefore we must regard each of them as requiring a small correction, and all these corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy every one of the equations expressing the relations between the various components of the group.

Thus it appears that the method required for adjusting the solar parallax and its related constants is in all respects the same as that which has so long been used for adjusting systems of triangulation, and as the latter method was invented by astronomers, it is natural to inquire why they have not applied it to the fundamental problem of their own science? The reasons are various, but they may all be classed under two heads. First, an inveterate habit of over-estimating the accuracy of our own work as compared with that of others; and second, the unfortunate effect of too much specialization.

The prevailing opinion certainly is that great advances have recently been made in astronomy, and so they have in the fields of spectral analysis and in the measurement of minute quantities of radiant heat; but the solution of the vast majority of astronomical problems depends upon the exact measurement of angles, and in that little or no progress has been made. Bradley, with his zenith sector a hundred and fifty years ago, and Bessel and Struve, with their circles and transit instruments seventy years ago, made observations not sensibly inferior to those of the present day, and indeed it would have been surprising if they had not done so. The essentials for accurately determining star places are a skilled observer, a clock and a transit circle, the latter consisting of a telescope, a divided circle and four micrometer microscopes. Surely no one will claim that we have to-day any more skilful observers than were Bessel, Bradley and Struve, and

the only way in which we have improved upon the telescopes made by Dollond one hundred and thirty years ago, is by increasing their aperture and relatively diminishing their focal distance. The most famous dividing engine now in existence was made by the elder Repsold seventy-five years ago; but as the errors of divided circles and their micrometer microscopes are always carefully determined, the accuracy of the measured angles is quite independent of any small improvement in the accuracy of the divisions or of the micrometer screws. Only in the matter of clocks has there been some advance, and even that is not very great. On the whole, the star places of to-day are a little better than those of seventy-five years ago, but even yet there is great room for improvement. One of the commonest applications of these star places is to the determination of latitude, but it is very doubtful if there is any point on the face of the Earth whose latitude is known certainly within one-tenth of a second.

Looking at the question from another point of view, it is notorious that the contact observations of the transits of Venus in 1761 and 1769 were so discordant that from the same observations Encke and E. J. Stone got respectively for the solar parallax 8.59 seconds and 8.91 seconds. In 1870 no one thought it possible that there could be any difficulty with the contact observations of the then approaching transits of 1874 and 1882, but we have found from sad experience that our vaunted modern instruments gave very little better results for the last pair of transits than our predecessors obtained with much cruder appliances in 1761 and 1769.

The theory of probability and uniform experience alike show that the limit of accuracy attainable with any instrument is soon reached; and yet we all know the fascination which continually lures us on in our efforts to get better results out of the familliar telescopes and circles which have constituted the standard equipment of observatories for nearly a century. Possibly these instruments may be capable of indicating somewhat smaller quantities than we have hitherto succeeded in measuring with them, but their limit cannot be far off, because they already show the disturbing effects of slight inequalities of temperature and other uncontrolable causes. So far as these effects are accidental they eleminate themselves from every long series of observations, but there always remains a residuum of constant error, perhaps quite unsuspected, which gives us no end of trouble. Encke's value of the solar parallax affords a fine illustration of this. From the transits of Venus in 1761 and 1769 he found 8.58

seconds in 1824, which he subsequently corrected to 8.57 seconds, and for thirty years that value was universally accepted. The first objection to it came from Hansen in 1854, a second followed from Le Verrier in 1858, both based upon facts connected with the lunar theory, and eventually it became evident that Encke's parallax was about one-quarter of a second too small.

Now please observe that Encke's value was obtained trigonometrically, and its inaccuracy was never suspected until it was revealed by gravitational methods, which were themselves in error about one-tenth of a second, and required subsequent correction in other ways. Here, then, was a lesson to astronomers, who are all more or less specialists, but it merely enforced the perfectly well known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Le Verrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements, and do everything possible to clear up the mystery which now surrounds the constant of aberration, but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question while he persistently neglected to adjust the net? And yet, until very recently, astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determinination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How then can we suppose that the result properly deducible from them can be materially affected by anything that any of us can do in a life time, unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determin-

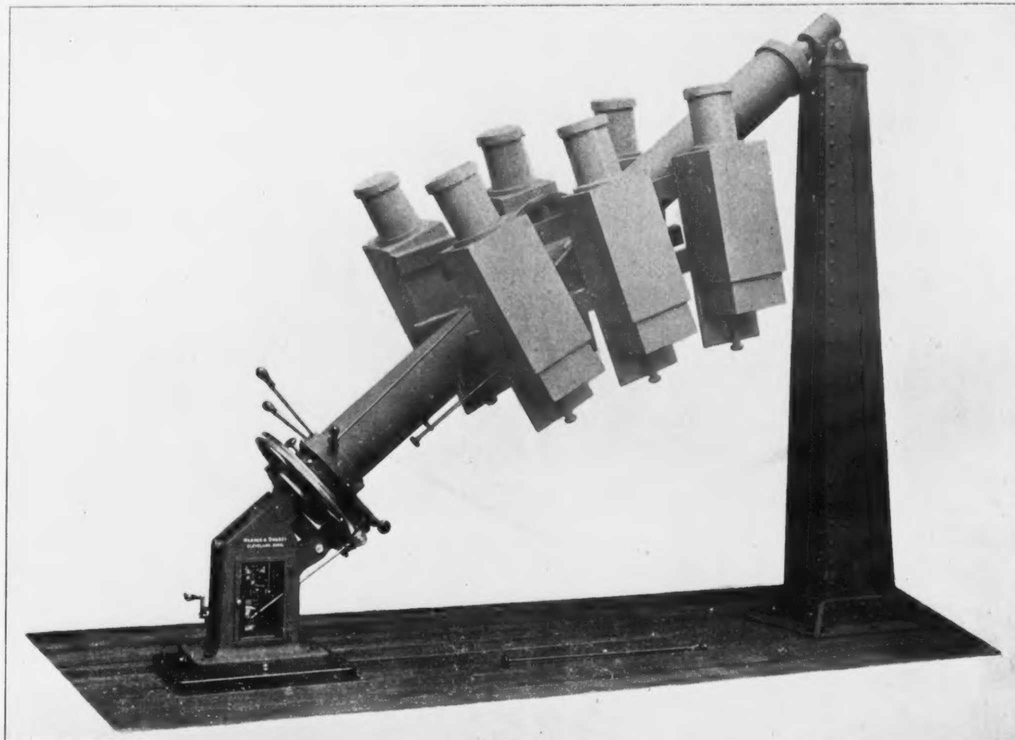
ation of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities, and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly $8.809'' \pm 0.0057''$, whence we have for the mean distance between the Earth and the Sun 92,797,000 miles, with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the planet Neptune, 5,578,400,000 miles.

INSTRUMENT FOR THE PHOTOGRAPHY OF METEORS FOR THE YALE OBSERVATORY.

W. L. ELKIN.

The experiments made at this Observatory last year seemed to show that if a sufficiently large field could be covered, it might be possible to secure quite a number of meteor tracks on photographic plates, during the August and December showers, at least. The incomparably greater accuracy, as against eye observations, with which these tracks locate the meteor and the radiant, has led us to consider the matter worth following up and accordingly application was made to the National Academy for an appropriation from the Lawrence Smith fund which is to be devoted to meteoric researches. From the grant awarded us the instrument represented in the cut has been constructed by Messrs. Warner and Swazey. It is a polar axis of the "English" form, this seeming to be the most convenient and the best adapted mounting for carrying a number of cameras, and admitting of long exposures without break. The axis is of tubular form, about 12 feet long, the ends being pivots working in bearings which are adjustable on their supports. The southern support, or base, contains the clock-work, the northern support is a column containing the driving weights, the connection being made by a cord passing under the floor. The declination axis carries arms on either end

PLATE XX.



INSTRUMENT FOR THE PHOTOGRAPHY OF METEORS FOR THE YALE OBSERVATORY.

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which serve as supports for the cameras. On the cut six dummy cameras are shown; it is not likely for the present, however, that we shall use more than four. Graduated circles and slow-motions for both coördinates are provided, and the clock-work has an electric control. The apparatus is now mounted here, and will be tried on the Perseids this year.

YALE UNIVERSITY OBSERVATORY.

THE LOCUS OF THE CENTRE OF GRAVITY FOR A HOMOGENEOUS ELLIPSOID OF REVOLUTION.*

T. J. J. SEE.

Herschel's "Outlines of Astronomy" and Airy's "Gravitation" have rendered a valuable service to science by furnishing the general reader, the student, the teacher, and even the theoretical astronomer, with a continuous representation of gravitational phenomena. The questions geometrically investigated by authorities like Newton, Herschel and Airy include the perturbations of the orbital motions of the heavenly bodies, and of their rotations about their centres of inertia; and Airy has even treated in a very elementary manner the attraction of an oblate planet. As the advantages of introducing geometrical representation into gravitational astronomy are so considerable that the procedure has been adopted by leading authorities in Celestial Mechanics, the writer is led to believe that a geometrical treatment of the attraction of a homogeneous ellipsoid of revolution will not be without interest.

It is to be understood here that the centre of inertia (or centre of mass) is a perfectly definite point, depending only upon the relative positions of the particles composing the body, and is fixed so long as the particles are relatively fixed. But the centre of gravity, being the point at which the whole mass might be collected and the attraction on a given point would be unchanged, is not fixed in the attracting mass, but depends upon the position of the attracted point relative to the attracting mass.

In case of symmetrical masses, the centre of inertia and the centre of gravity will occasionally coincide, but in general the centre of gravity will not be in the same direction as the centre of inertia, nor at the same distance.

* Read before the Chicago Academy of Sciences, Sept. 4, 1894.

As a concrete example, we may consider the attraction of an oblate planet: the body will rotate about its centre of inertia, or centre of figure (when homogeneous), but the force of gravity is normal to the surface of equilibrium, and therefore does not generally pass through the centre of figure, or centre of inertia.

It is well known from the theory of the figures of the planets that oblateness renders the attraction in the direction of the pole less than if the whole mass were collected at the centre of figure, while the attraction in the direction of the equator is rendered correspondingly greater; and from this it follows that at some intervening latitude the attraction must be of the same intensity as if the whole mass were collected at the centre. For a homogeneous ellipsoid of revolution differing but little from a sphere this latitude is found to be

$$\psi = 35^\circ\, 15'\, 52''$$

(See Laplace's *Mècanique Cèleste*, Tome II, p. 110, or Tisserand's *Mècanique Cèleste*, Tome II, p. 66). But we observe that as the attraction is always normal to the surface of equilibrium it is not directed toward the centre of the ellipsoid, but in the tangent to the evolute. As the body is supposed to be an ellipsoid of revolution, we need regard only a section of the meridian, which will be an ellipse whose equation is

$$\frac{\xi^2}{a^2} + \frac{\eta^2}{b^2} = 1,$$

where a is the semi major and b the semi-minor axis of the ellipsoid. If we introduce $\lambda = \tan\varphi$, where φ is the angle of the eccentricity determined by $e = \sin\varphi$, we may write the equation for the ellipse in the form

$$\frac{\xi^2}{1+\lambda^2} + \eta^2 = b^2. \qquad (1)$$

The evolute of the ellipse is given by the equation

$$\frac{\xi^{\frac{2}{3}}}{A^{\frac{2}{3}}} + \frac{\eta^{\frac{2}{3}}}{B^{\frac{2}{3}}} = 1, \qquad (2)$$

where

$$A = \frac{a^2 - b^2}{a}, \quad B = \frac{a^2 - b^2}{b}.$$

(See *Salmon's Conic Sections*, 5th edition, p. 220).

By means of this equation we may draw the curve for an ellipse of any given eccentricity.

If now we put σ for the density of the ellipsoid, $\lambda = \tan\varphi$, and

$$P = 4\pi\sigma \frac{1+\lambda^2}{\lambda^3} (\lambda - \text{arc tan } \lambda), \tag{3}$$

then, it is shown in Tisserand's *Mecanique Celeste*, Tome II, p. 88, that the attraction at any point on the surface of the ellipsoid is given by the equation

$$g = \frac{Pb^2}{\delta}, \tag{4}$$

where δ is the perpendicular distance from the centre of the ellipsoid to the plane tangent to the ellipsoid at the given point.

It is easily shown that δ is given by the equation

$$\delta = \frac{b^2}{\sqrt{\eta^2 + \frac{\xi^2}{(1+\lambda^2)^2}}} \tag{5}$$

Now by equation (3) we see that P is constant, and of course b is constant; therefore by (4) we learn that the force of gravity in different latitudes varies inversely as the perpendicular distance δ from the centre to the tangent plane; and hence it is evident that the attraction in the direction of the pole will be greater than that in the direction of the equator.

This result seems inconsistent with that previously stated, viz.: The attraction in the direction of the pole is less than if the whole mass were collected at the centre of the figure, while in the direction of the equator it is greater.

The apparent discordance is due to the increase of distance at the equator due to oblateness, which makes the *centre of gravity* there more remote from the surface than it is at the pole, notwithstanding the fact that at the equator the centre of gravity is (as we shall see hereafter) between the centre of figure and the surface, while at the pole it is beyond the centre of figure.

We shall now deduce the locus of the center of gravity as the attracted point moves along the surface from the pole to the equator.

In the first place we know the centre of gravity will be on the normals at the different points of the quadrant.

As a graphical illustration is desired, we chose an ellipsoid of considerable oblateness, so that the evolute and locus may be sufficiently large when drawn to scale. We take the oblateness as 0.1, which about corresponds to the planet Saturn, and then the eccentricity $e = 0.436$; with a semi-major axis of convenient length (10 inches) we draw an ellipse which will represent a meridianal section of the ellipsoid.

By means of equation (1) we compute the values of η corresponding to convenient arguments of ξ; and with these values of the co-ordinate η and ξ, we compute the evolute by means of equation (2), and plat it to scale. The normals at the different points of the quadrant are then drawn tangent to the evolute.

It remains to compute δ from equation (5), and thus we obtain tabular members which are inversely proportional to the attraction at the different points.

Now
$$g = \frac{Pb^2}{\delta}$$

and we learn from the theory of the attraction of an ellipsoid (Tisserand's *Mecanique Celeste*, Tome II, p. 66) that in latitude

$$\psi = 35^\circ\ 15'\ 52'',$$

$$g = \frac{Pb^2}{\delta_1} = \frac{M}{r_1^2};$$

where M is the mass of a sphere having the same volume and density, and r_1, its radius. Since δ_1 and r_1, are both perpendicular to the tangent plane, and therefore parallel, and r_1 is also sensibly equal to the distance of the attracted point from the centre of the ellipsoid (Tisserand's *Mecanique Celeste*, Tome II, p. 66; quantities of the order λ^4 are here neglected); it follows that r_1 and δ_1 are sensibly equal.

Now we may take the average force of gravity ($\psi = 35^\circ\ 15'\ 52''$) as the unit, and then

$$g_1 = \frac{Pb^2}{\delta_1} = \frac{M}{r_1^2} = 1.$$

Since δ is at this particular point sensibly equal to r_1 we may unite

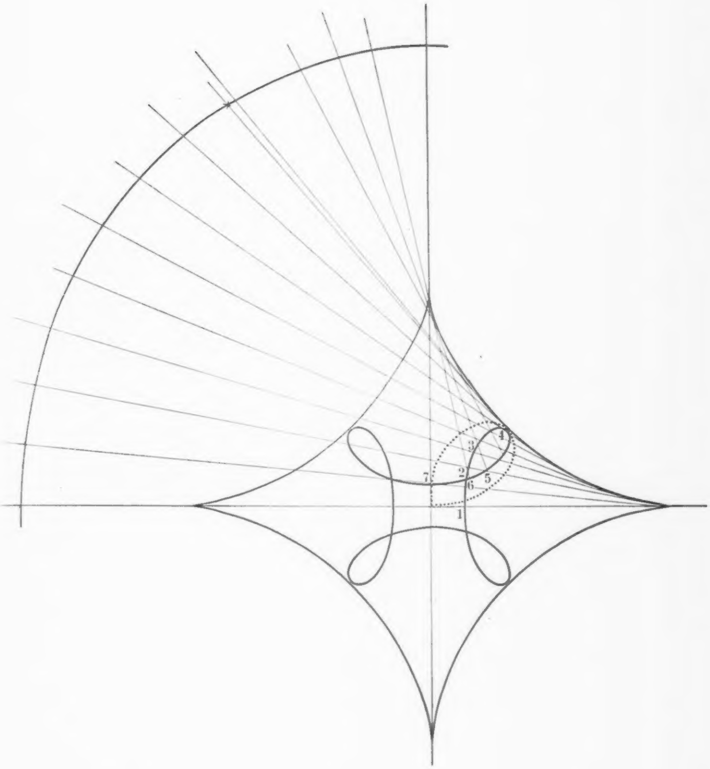
$$\frac{M}{\delta_1^2} = \frac{Pb^2}{\delta_1}, \text{ or } \delta_1 = \frac{M}{Pb^2} = 1;$$

and hence

$$\left(\frac{\delta}{\delta_1}\right) = \frac{r^2}{r_1^2} = \frac{r^2}{\delta_1^2}, \text{ or } \delta\delta_1 = r^2;$$

but since $\delta_1 = 1$, we have $r = \sqrt{\delta}$, where δ is expressed in terms of r_1 or δ_1, as the unit. That is, the distance of the centre of gravity from the surface along the normals is equal to the square root of δ expressed in terms of the mean radius r_1 as the unit. In case of the ellipsoid illustrated in the figure, $a = 10$, and $r_1 = 9.6549$; the numerical values of r found from $r = \sqrt{\delta}$, must therefore be multiplied by $9.6549 = r_1$ in order to reduce them to the scale

PLATE XXI.



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$a = 10$. The quadrant of the ellipse shown in the figure is reduced by the factor $\frac{2}{5}$, so that it is on a smaller scale than the other curves.

When the values of r have been found, we plat the resulting points along the normals and obtain the looped curve indicated in the figure. The course of the curve in the other three quadrants is similar, and we find for the entire locus the beautiful curve with four loops.

It will be seen that the curve is very symmetrical and regular, but of a peculiar shape. Now as the point moves from the pole to the equator, the centre of gravity moves along the curve 1, 2, 3, 4, 5, 6, 7.

Thus we see that for points near the pole (where $\psi > 35° 15' 52''$), the centre of gravity is remoter than the centre of figure, and therefore the attraction is less than if the mass were collected at the centre of figure; for points near the equator (where $\psi < 35° 15' 52''$) the centre of gravity is nearer than the centre of figure, and hence the attraction is greater than if the whole mass were collected at that point. The distance of the points on the surface from the centre of the ellipsoid, when measured on the normals, gives the pointed line, which is seen to approach the locus at the point most remote from the centre, which corresponds to $\psi = 35° 15' 52''$. The distance of the pointed line from the locus measured along the normals, represents the amount by which the centre of gravity is nearer or remoter (relative to points on the surface) than the centre of figure.

By this simple geometrical device we are enabled to study the law of attraction of a homogeneous ellipsoid or planet, and it only remains to add that the locus of the centre of gravity for the planet's *surface* will be the *surface* resulting from the revolution of the curve here drawn around the axis of revolution of the ellipsoid.

This doubly-intersecting surface is rather complex; and we easily see that its complication is due to the fact that the locus of the centre of gravity is a function of the surface of the ellipsoid, which in turn depends upon the law of gravitation. Though we have thus far referred the locus of the centre of gravity to the surface of the ellipsoid, it is easy to refer the curve to the rectangular axes in the following manner:

If ψ denote the angle made by the normal with the plane of the equator, and r be the distance of any point (ξ, η) on the surface from the centre of gravity, and ξ' and η' denote the coördinates of the centre of gravity referred to the centre of the ellipse; then we shall have

$$\begin{aligned} \xi - \xi' &= r\cos\psi, \\ \eta - \eta' &= r\sin\psi; \text{ or} \end{aligned} \qquad \left.\begin{aligned} \xi' &= \xi - r\cos\psi, \\ \eta' &= \eta - r\sin\psi \end{aligned}\right\} \quad (6)$$

The angle ψ may be found by means of its tangent, which results from dividing the ordinate η by the subnormal. The subnormal of the ellipse is $-\frac{b^2}{a^2}\xi$, (the negative sign may here be omitted, as it merely indicates that the subnormal is to be taken towards the origin), and hence we have

$$\tan\psi = \left(\frac{a}{b}\right)^2 \cdot \frac{\eta}{\xi} \qquad (7)$$

Hence ψ is at once found from the values of ξ and η; and r being found as previously indicated, we at once find the coördinates ξ' and η', which give the locus relative to the rectangular axes. Equations (6) are therefore of interest, even if they do not diminish the numerical work involved in tracing the curve.

By such a figure we see not merely the variation in the direction of gravity, but also its intensity relative to its average value, and hence it may be hoped that some new light is thrown upon the attraction of the planets which could not be gathered from purely analytical formulæ.

THE UNIVERSITY OF CHICAGO, 1894, Aug. 23.

SCHIAPARELLI'S LATEST VIEWS REGARDING MARS.*

WILLIAM H. PICKERING.

It is probable that the astronomer whose name is most closely linked with the planet Mars at the present time is Giovanni Schiaparelli. And yet although nearly everybody has heard of Schiaparelli's canals, very few astronomers even, outside of France and Italy, had until recently more than a very vague notion what were really his ideas in regard to them. This is due probably to the fact that he has written exclusively in Italian, a language which very few American astronomers, and I believe very few English ones, understand. To this fact chiefly I think is due the great incredulity with which his observations have been treated, at least until recently, in both of these countries. Astronomers could understand his maps, they knew therefore what he had done, but they could not understand his descriptions of his observations, and so were incredulous regarding their accur-

* Communicated by the author.

acy. Moreover, such a mass of detail appeared upon his maps, which had not before been seen by others, that it completely masked the more striking features of the planet, thus rendering its appearance entirely different from that which it presented in the telescope under ordinary atmospheric conditions.

But within the last few years a change has occurred. Flammarion has translated a large part of Schiaparelli's writings into French, a language with which most English speaking astronomers are familiar, and moreover the canals have been seen by a number of astronomers whose descriptions of them in English and French could be understood, and were found to agree with those of Schiaparelli.

But errors are still frequently made by people who might be expected to know better. Thus, many people suppose that Schiaparelli was the original discoverer of the canals, a claim which he never made for himself. In point of fact some of them appear upon maps of the planet published more than fifty years ago. The former English incredulity in the matter seems the more strange, since many of the canals were seen by Dawes in 1864, and by Burton and Dreyer in 1879. Schiaparelli however has discovered far more canals than anyone else, and he is also the discoverer of their gemination.

In this connection, it may be that a brief chronological statement of the more important facts and discoveries relating to Mars will not be without interest. In compiling it I have been chiefly indebted to Flammarion's classic work "La Planète Mars," although other sources have also been consulted.

272 B. C. The first known observation of Mars is recorded in Ptolemy's Almagest.

1610. The phases of Mars were discovered by Galileo.

1659. The first sketch showing surface detail was made by Huyghens. He also suggested a rotation in 24 hours.

1666. Cassini determined the rotation of Mars to take place in 24 hours 40 minutes. He also observed the polar caps, and "he distinguished on the disc of Mars, near the terminator, a white spot advancing into the dark portion, and representing without doubt, like those of the Moon, a roughness or irregularity of the surface." This latter statement is curious, but the effect was undoubtedly due to irradiation, since his telescope was entirely inadequate to enable him to observe such a delicate phenomenon.

1777. With the exception of Huyghens, Hooke, and possibly Maraldi, no one succeeded in making recognizable sketches of the

surface detail upon Mars for over a century, until Sir William Herschel took the matter up in this year.

1783. Sir William Herschel detected the variation of the size of the polar snow caps with the seasons, measured the polar compression, and determined the inclination of the axis of the planet to its orbit.

1785-1802. Schrœter made an extended study of the planet. His drawings are upon the whole rather better than those of Herschel. He discovered among other things the very dark spots on which I have referred in my publications as the Northern and Equatorial Seas. He however supposed them to be clouds.

1840. Beer and Maedler published the first map of the planet, assigning latitudes and longitudes to the various markings. On this map are indicated the first canals, and the first of the small lakes, so many of which have been discovered during the last few years. The canals are *Nectar* and *Agathodaemon* and portions of *Hades* and *Tartarus*. The lake is *Lacus Phoenicis*. Their map is the first satisfactory representation of the entire surface of the planet. The only region which previous observers had clearly distinguished was that in the vicinity of the *Syrtis Major*.

1858. Secchi made a careful study of the colors exhibited by the planet.

1862. Lockyer made the first series of really good sketches of the planet, showing all the characteristic forms with which we are now so familiar. His drawings, and also those of some of the other observers, give the first indications of the appearance of the central branch in the Y, so called by Secchi.

1864. Dawes detected eight or ten of the canals.

1867. Huggins detected lines due to the presence of water vapor in the spectrum of Mars.

1867. Proctor determined the period of rotation of Mars within 0.1 second.

1877. Hall discovered the two satellites of Mars.

1877. Green made a very excellent series of drawings of the planet, superior to anything which had preceded them.

1877. Schiaparelli made the first extensive triangulation of the surface of the planet, and added very largely to the number of known canals.

1879. Schiaparelli detected the gemination of *Nilus*,—the first known double canal.

1882. Schiaparelli discovered numerous double canals, and announced that the appearance formed one of the characteristic phenomena of the planet.

The most reliable confirmation of this phenomenon hitherto reported has come from Perrotin of Nice, and A. Stanley Williams in England. If Schiaparelli's theory is correct, that the duplication occurs only between the spring and autumn equinoxes of the northern hemisphere, the last opportunity to witness it was in 1890, and the next will be in January and February of 1895, unless the planet proves to be too remote at that period.

Very few of Schiaparelli's writings have ever been translated into English, and none so far as I know, hitherto, without the intervention of some other language, such as German or French. The following translation is from "Natura ed Arte" for February 15, 1893. It gives the latest expression of his views upon the periodical inundations experienced by the planet, upon the nature of the seas, the canals, and the gemination of the latter.

LOWELL OBSERVATORY, Flagstaff, Arizona,
August 25, 1894.

THE PLANET MARS.

GIOVANNI SCHIAPARELLI.

Many of the first astronomers who studied Mars with the telescope, had noted on the outline of its disc two brilliant white spots of rounded form and of variable size. In process of time it was observed that whilst the ordinary spots upon Mars were displaced rapidly in consequence of its daily rotation, changing in a few hours both their position and their perspective, that the two white spots remained sensibly motionless at their posts. It was concluded rightly from this, that they must occupy the poles of rotation of the planet, or at least must be found very near to them. Consequently they were given the name of polar caps or spots. And not without reason is it conjectured, that these represent upon Mars that immense mass of snow and ice, which still to-day prevents navigators from reaching the poles of the Earth. We are led to this conclusion not only by the analogy of aspect and of place, but also by another important observation.

* * * * * * * * * * * * *

As things stand, it is manifest, that if the above mentioned white polar spots of Mars represent snow and ice, they should continue to decrease in size with the approach of summer in those places, and increase during the winter. Now this very fact is observed in the most evident manner. In the second half of the year

1892 the southern polar cap was in full view; during that interval, and especially in the months of July and August, its rapid diminution from week to week was very evident, even to those observing with common telescopes. This snow, (for we may well call it so,) which in the beginning reached as far as latitude 70°, and formed a cap of over 2000 kilometers (1200 miles) in diameter, progressively diminished, so that two or three months later little more of it remained than an area of perhaps 300 kilometers, (180 miles) at the most, and still less was seen later in the last days of 1892. In these months the southern hemisphere of Mars had its summer; the summer solstice occuring upon October 13. Correspondingly the mass of snow surrounding the northern pole should have increased; but this fact was not observable, since that pole was situated in the hemisphere of Mars which was opposite to that facing the Earth. The melting of the northern snow was seen in its turn in the years 1882, 1884 and 1886.

These observations of the alternate increase and decrease of the polar snows are easily made, even with telescopes of moderate power, but they become much more interesting and instructive when we can follow assiduously the changes in their more minute particulars, using larger instruments. The snowy regions are then seen to be successively notched at their edges; black holes and huge fissures are formed in their interiors; great isolated pieces many miles in extent stand out from the principal mass, and dissolving, disappear a little later. In short, the same divisions and movements of these icy fields present themselves to us, at a glance, that occur during the summer of our own arctic regions, according to the descriptions of explorers.

The southern snow, however, presents this peculiarity, that the center of its irregularly rounded figure does not coincide exactly with the pole, but is situated at another point, which is nearly always the same, and is distant from the pole about 300 kilometers (180 miles) in the direction of the *Mare Erythraeum*. From this we conclude that when the area of the snow is reduced to its smallest extent, that the south pole of Mars is uncovered; and therefore perhaps, the problem of reaching it upon this planet is easier than upon the Earth. The southern snow is in the midst of a huge dark spot, which with its branches occupies nearly one third of the whole surface of Mars, and is supposed to represent its principal ocean. Hence the analogy with our arctic and antarctic snows may be said to be complete, and especially so with the antarctic one.

The mass of the northern snow-cap of Mars is on the other

hand centered almost exactly upon its pole. It is located in a region of yellow color, which we are accustomed to consider as representing the continent of the planet. From this arises a singular phenomenon which has no analogy upon the Earth. At the melting of the snows, accumulated at that pole during the long night of ten months and more, the liquid mass produced in that operation is diffused around the circumference of the snowy region, converting a large zone of surrounding land into a temporary sea, and filling all the lower regions. This produces a gigantic inundation, which has led some observers to suppose the existence of another ocean in those parts, but which does not really exist in that place, at least as a permanent sea. We see then, (the last opportunity was in 1884), the white spot of the snow surrounded by a dark zone, which follows its perimeter in its progressive diminution, upon a circumference ever more and more narrow. The outer part of this zone branches out into dark lines, which occupy all the surrounding region, and seem to be distributary canals, by which the liquid mass may return to its natural position. This produces in these regions very extensive lakes, such as that designated upon the map by the name of *Lacus Hyperboreus*; the neighboring interior sea called *Mare Acidalium* becomes more black, and more conspicuous. And it is to be remembered as a very probable thing, that the flowing of this melted snow is the cause which determines principally the hydrographic state of the planet, and the variations that are periodically observed in its aspect. Something similar would be seen upon the Earth, if one of our poles came to be located suddenly in the center of Asia or of Africa. As things stand at present, we may find a miniature image of these conditions in the flooding that is observed in our streams at the melting of the Alpine snows.

Travellers in the arctic regions have frequent occasion to observe how the state of the polar ice at the beginning of the summer, and even at the beginning of July is always very unfavorable to their progress. The best season for exploration is in the month of August, and September is the month in which the trouble from the ice is the least. Thus in September our Alps are usually more practicable than at any other season. And the reason for it is clear, the melting of the snow requires time, a high temperature is not sufficient, it is necessary that it should continue, and its effect will be so much the greater, as it is the more prolonged. Thus, if we could slow down the course of our seasons, so that each month should last sixty days instead of

thirty, in the summer in such a lengthened condition, the melting of the ice would progress much further, and perhaps it would not be an exaggeration to say that the polar cap at the end of the warm season would be entirely destroyed. But one cannot doubt in any case, that the fixed portion of such a cap would be reduced to much smaller size, than we see it to-day. Now this is exactly what happens in Mars. The long year, nearly double our own, permits the ice to accumulate during the polar night of ten or twelve months, so as to descend in the form of a continuous layer as far as parallel 70°, or even further. But in the day which follows of twelve or ten months, the Sun has time to melt all, or nearly all, of the snow of recent formation, reducing it to such a small area, that it seems to us no more than a very white point. And perhaps this snow is entirely destroyed, but of this there is at present no satisfactory observation.

Other white spots of a transitory character, and of a less regular arrangement are formed in the southern hemisphere, upon the islands near the pole, and also in the opposite hemisphere, whitish regions appear at times surrounding the north pole, and reaching to 50° and 55° of latitude. They are perhaps transitory snows, similar to those which are observed in our latitudes. But also in the torrid zone of Mars are seen some very small white spots more or less persistent, amongst others one was seen by me in three consecutive oppositions (1877-1882) at the point indicated upon our chart by longitude 268°, and latitude 16° north. Perhaps we may be permitted to imagine in this place the existence of a mountain capable of supporting extensive ice-fields. The existence of such a mountain has been supposed also by some recent observers, founded upon other facts.

As has been stated, the polar snows of Mars prove in an incontrovertable manner, that this planet, like the Earth, is surrounded by an atmosphere capable of transporting vapor from one place to another. These snows are in fact precipitations of vapor, condensed by the cold, and carried with it successively. How carried with it, if not by atmospheric movement? The existence of an atmosphere charged with vapor has been confirmed also by spectroscopic observations, principally those of Vogel; according to which this atmosphere must be of a composition differing little from our own, and above all *very rich in aqueous vapor.* This is a fact of the highest importance, because from it we can rightly affirm with much probability, that to water, and to no other liquid is due the seas of Mars and its polar snows. When this conclusion is assured beyond all doubt, another one

may be derived from it, of not less importance,—that the temperature of the Arean climate, notwithstanding the greater distance of that planet from the Sun, is of the same order as the temperature of the terrestrial one. Because, if it were true, as has been supposed by some investigators, that the temperature of Mars was on the average very low, (from 50° to 60° below zero!) it would not be possible for water vapor to be an important element in the atmosphere of that planet, nor could water be an important factor in its physical changes; but would give place to carbonic acid, or to some other liquid whose freezing point was much lower.

The elements of the meteorology of Mars seem then to have a close analogy to those of the Earth. But there are not lacking, as might be expected, causes of dissimilarity. From circumstances of the smallest moment, nature brings forth an infinite variety in its operations. Of the greatest influence must be the different arrangement of the seas and the continents upon Mars, and upon the Earth, regarding which, a glance at the map will say more than would be possible in many words. We have already emphasized the fact of the extraordinary periodical flood, which at every revolution of Mars inundates the northern polar region at the melting of the snow. Let us now add that this inundation is spread out to a great distance by means of a network of canals, perhaps constituting the principal mechanism (if not the only one) by which water (and with its organic life) may be diffused over the arid surface of the planet. Because on Mars it rains very rarely, *or perhaps even, it does not rain at all.* And this is the proof.

Let us carry ourselves in imagination into celestial space, to a point so distant from the Earth, that we may embrace it all at a single glance. He would be greatly in error, who had expected to see reproduced there, upon a great scale, the image of our continents with their gulfs and islands, and with the seas that surround them, which are seen upon our artificial globes. Then without doubt the known forms, or part of them, would be seen to appear under a vaporous veil, but a great part (perhaps one half) of the surface would be rendered invisible, by the immense fields of cloud, continually varying in density, in form and in extent. Such a hindrance, most frequent and continuous in the polar regions, would still impede nearly half the time the view of the temperate zones, distributing itself in capricious and ever varying configurations. The seas of the torrid zone would be seen to be arranged in long parallel layers, corresponding to the

zone of equatorial and tropical calms. For an observer placed upon the Moon, the study of our geography would not be so simple an undertaking as one might at first imagine.

There is nothing of this sort in Mars. In every climate, and under every zone, its atmosphere is nearly perpetually clear, and sufficiently transparent to permit one to recognize at any moment whatever, the contours of the seas and continents, and more than that, even the minor configurations. Not indeed that vapors of a certain degree of opacity are lacking, but they offer very little impediment to the study of the topography of the planet. Here and there we see appear from time to time a few whitish spots, changing their position and their form, rarely extending over a very wide area. They frequent by preference a few regions, such as the islands of the *Mare Australe*, and on the continents, the regions designated on the map with the names of *Elysium* and *Tempe*. Their brilliancy generally diminishes and disappears at the meridian hour of the place, and is reinforced in the morning and evening, with very marked variations. It is possible that they may be layers of cloud, because the upper portions of terrestrial clouds, where they are illuminated by the Sun, appear white. But various observations lead us to think that we are dealing rather with a thin veil of fog, instead of a true nimbus cloud, carrying storms and rain. Indeed it may be merely a temporary condensation of vapor, under the form of dew or hoar frost.

Accordingly, as far as we may be permitted to argue from the observed facts, the climate of Mars must resemble that of a clear day upon a high mountain. By day a very strong solar radiation hardly mitigated at all by mist or vapor, by night a copious radiation from the soil towards celestial space, and because of that a very marked refrigeration. Hence a climate of extremes, and great changes of temperature from day to night, and from one season to another. And as on the Earth at altitudes of 5,000 and 6,000 meters (17,000 to 20,000 feet), the vapor of the atmosphere is condensed only into the solid form, producing those whitish masses of suspended crystals, which we call cirrus clouds, so in the atmosphere of Mars, it would be rarely possible (or would even be impossible) to find collections of cloud capable of producing rain of any consequence. The variation of the temperature from one season to another would be notably increased by their long duration, and thus we can understand the great freezing and melting of the snow, which is renewed in turn at the poles at each complete revolution of the planet around the Sun.

(TO BE CONTINUED.)

ELECTRICAL CONTROL OF EQUATORIALS IN PHOTOGRAPHY.

W. C. GURLEY.

In the January number of this journal appeared an article by Professor W. H. Pickering upon the subject of "Telescope Mountings and Domes," in which the author calls attention to a method employed at Greenwich for correcting the rate of the Standard Mean Time clock in that Observatory, and suggests that a somewhat similar arrangement might be advantageously adopted for the correction of driving clocks of equatorials when used for photographic work.

Acting upon the suggestion of Professor Pickering the writer during the past winter has made a number of experiments looking to this end.

The clock placed under electric control was one attached to the refractor of Marietta College Observatory, an exceptionally well made Bond Spring Governor by the Howards of Boston.

Much abuse has been heaped upon this time-honored form of driving-clock regulation, but it has always seemed to the writer that for accuracy of results and thorough reliability this governor has never been surpassed.

In the final trial of this electrical method of control two permanent bar magnets, three inches long, five eighths of an inch wide, and one quarter inch thick were attached vertically to opposite sides of the pendulum bob—their north and south poles reversed; half an inch below the lower end of the permanent magnets, two electro-magnets were placed and so wound that a current sent in a given direction would render them north and south seeking poles respectively—Four Leclanché cells connected in series, a switch for reversing the battery current, a pear push button included in the circuit, and brought to the eye-end of the telescope, completed the arrangement.

It was found upon sending a current in such a direction as to cause the electro-magnets to assume unlike polarity with the permanent magnets above them, that the clock would be accelerated two beats, or one second in sixty. A reverse current producing an opposite effect, and causing the clock to lose one second per minute. All this can be done easily, without jar or tremor, and is completely under the control of the observer.

Of course the change of position of the image of an object upon the sensitive plate due to refraction in declination is not affected by this control, but in case of the smaller instruments this correction can be applied by means of the clamp and tangent.

PHOTOGRAPH OF SWIFT'S NEBULA IN MINOCEROS, N. G. C. 2237.*

E. E. BARNARD.

I send for reproduction in ASTRONOMY AND ASTRO-PHYSICS an enlargement of my photograph of this mixture of nebulosity and stars.

The picture was made with the 6-inch Willard lens, 1894, January 11^{h} 7^{h} 47^{m} to 11^{h} 3^{m} standard Pacific Time and is enlarged 2.8 times.

It is a fine example of the free mixture of stars and nebulosity to which I have called attention in my paper on "Photographic Nebulosities and Star Clusters Connected with the Milky Way" in ASTRONOMY AND ASTRO-PHYSICS for March, 1894.

Though the cluster (G. C. 1420) is apparently involved bodily in this nebula it will be readily seen that there is no tendency of the nebulosity to condense about the individual stars. It will be interesting to compare this picture with any recent photograph of the Pleiades, that for instance of Dr. Wilson, recently printed in ASTRONOMY AND ASTRO-PHYSICS. This will show the striking contrast between the two classes of nebulous clusters.

In the Pleiades the nebulosity is strongly condensed about the individual bright stars, while in the present picture there is no such tendency to condensation—the stars are seemingly freely mixed with the nebulosity. Of course it is possible their apparent intermixture may be due only to projection of the stars on the nebulosity, but this is highly improbable.

I send with this also for reproduction my sketch of this object that appeared in *A. N.* Vol. 122. This is to be reduced to $\frac{1}{2\frac{1}{2}}$ its size to be on the same scale as the photograph. For completeness, I will also quote what I have previously written about this nebula in ASTRONOMY AND ASTRO-PHYSICS for March, 1894.

"In *A. N.* 2918 I have given an account, along with a sketch, of a large nebulous ring enclosing the cluster G. C. 1420, the nebula itself being N. G. C. 2237. The place of G. C. 1420 for 1860.0 is $6^{h}\,23^{m}\,29^{s} + 5°\,2'.5$.

The nebula (2237) was discovered by Swift very many years ago, and was independently found by me in January, 1883.

The sketch referred to was made with the 12-inch in 1889.

I have recently (Jan. 9, 1894,) secured a fine photograph of this object with the Willard lens. As stated in *A. N.* 2918, from its

* Communicated by the author.

PLATE XXI*a*.

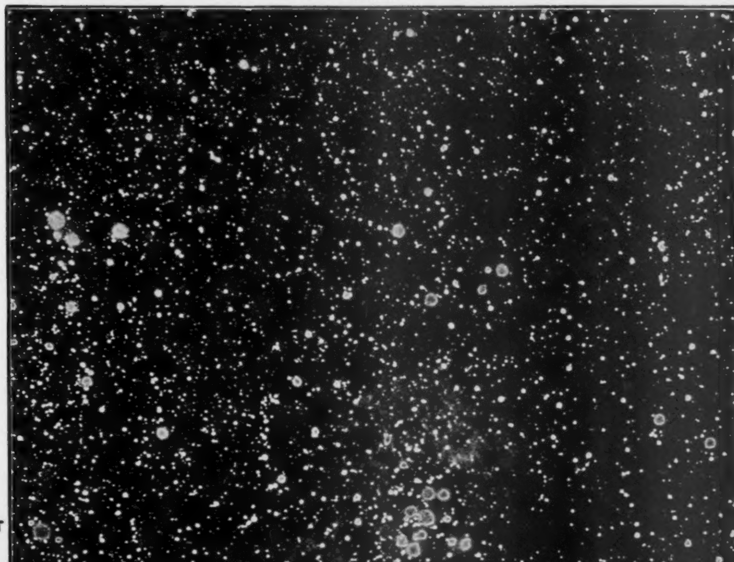
PHOTOGRAPH OF SWIFT'S NEBULA IN MONOCEROS,

1894. January 11, $7^h\ 47^m - 11^h\ 3^m$ S. P. T.

Made with the 6 in. Willard lens of the Lick Observatory, by E. E. Barnard.

NORTH

EAST

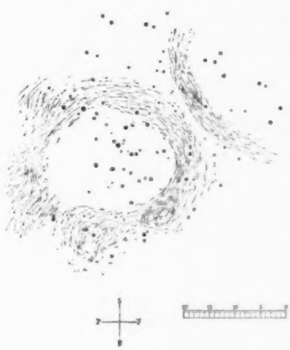


WEST

SOUTH

XUM

diffused nature this nebula is specially suited for photography. This photograph verifies the statement most emphatically and shows how utterly impossible it is to adequately deal with such an object visually.



I will quote from my previous article on this subject in *A. N.* 2918, in speaking of its appearance with the 12-inch in 1889:

"What I had seen previously and what Swift had sketched, was simply a brightish knot in a vast nebulous ring that entirely surrounded the cluster. By estimation the average outer diameter of the ring is 40′ and the inner diameter 20′. The inside of the ring is apparently free of nebulosity, the stars of the cluster shining on a perfectly dark sky. The outer edge of the ring is somewhat diffused and irregular, some projections occurring near the following portion. The inner edge is more definite and especially so following—it is less definite in the preceding part. In the north preceding section of the ring are several knots, the largest of which, *a*, is the one previously seen by Swift and myself. I am not sure that there is not a very small break in the continuity of the ring at the point *b*. South following the ring and close to it is the nebulous section of a large ellipse which seems to be a portion of another great ring; I am not sure that this is not connected with the first by a nebulous strip."

Comparing the photograph with my sketch, I find my sketch is correct so far as it goes, but with the 12-inch I had grasped only the brighter details—the great mass of it not being seen.

The photograph shows the nebula to be about 1° in diameter and very irregular in brightness and outline. It is a mass of unequally condensed nebulosity involving the star cluster and specially heavy north of the bright stars. The nebulous knots or condensations, shown in my sketch, are conspicuous on the photographs, as is also the "nebulous section of a large ellipse," which is connected with the main mass—the full extent of this section was shown in the sketch.

The photograph shows that there is no nebulosity—or if any it is very feeble—immediately about most of the bright stars. They apparently shine in a vacant space in the south part of the nebula.

The entire object seems to be definitely terminated and to leave no suggestion of a greater extent being revealed through a prolonged exposure.

One degree south of the center of the nebula, and free of it, and following about ¼°, is a very thin nebulous strip 10′ or 12′ long extending north and south with a faint star in its south end, like a slender comet with a nucleus.

THE REGION OF LACUS SOLIS ON MARS.

J. M. SCHAEBERLE.

During some unusually fine seeing on the morning of Sunday Sept. 2, 1894, at 2:30 A. M., Locus Solis was very clearly shown to be composed of three separate areas. Each area was very dark. The two preceding areas are elongated in a north and south direction and are enveloped and connected by a penumbral shade; the third area (following) is round and quite disconnected from the general mass. The five very small and black circular areas nearer to the planet's equator (shown in the sketch inclosed within the bounding square) were connected by an intensely black and very narrow continuous line which passed centrally through each area, but did not extend to the familiar black circular area from which several so-called "canals" radiate —only the usual faint and diffused marking forming the continuation of the line.

In this connection attention should be called to the fact that in June, 1890, Schiaparelli saw Locus Solis divided into two parts the advancing area being the smaller of the two. (See *La Planéte Mars*, by Camille Flammarion, page 475.)

LICK OBSERVATORY, Sept. 3, 1894.

CORRECTIONS TO THE N.G.C. OF NEBULÆ. *H. Kobold.*

It is suggested that N.G.C 3760, only seen once by d'Arrest, and not found again nor seen by anyone else, may be affected by an error of one hour in R. A. and be = 3301 = II. 46. The group 3745, 46, 48, 50, 51, 53, 54, should all be corrected by + 1^m 32^s and − 15′.9. They were found by Copeland, with Lord Rosse's telescope, but an error was afterwards made in identifying the comparison star.—*A. N.* 3241.

ASTRONOMY AND ASTRO-PHYSICS.

PLATE XXII.

1892 Aug. 29, $8^h\ 0^m$ P. s. t.
λ 290°, β — 12°.

1892 Aug. 27, $10^h\ 10^m$ P. s. t.
λ 339°, β — 12°.

1892 Aug. 24, $11^h\ 0^m \pm$ P. s. t.
λ 18°, β —12°.

1892 Aug. 20, $11^h\ 0^m$ P. s. t.
λ 54°, β — 12°.

(Lacus Solis. 1894 Sept. 1, $14^h\ 5^m$)
1892 Aug. 14, $11^h\ 50^m$ P. s. t.
λ 120°, β — 12°.

1892 Aug. 8, $9^h 30^m - 10^h 5^m$ P.s.t.
λ 139°, β — 12°.

1892 Aug. 7, $11^h\ 0^m – 11^h\ 45^m$ P.s.t.
λ 170°, β — 12°.

1892 Aug. 7, $13^h\ 0^m$ P. s. t.
λ 189°, β — 12°.

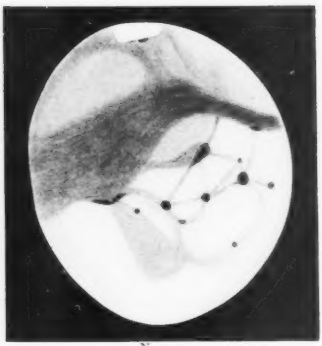
1892 Juli 31, $13^h\ 30^m$ P. s. t.
λ 268°, β — 13°.

PLATE S^2. — DRAWINGS OF MARS, 1892.

By Professor SCHAEBERLE.

PLATE XXIII.

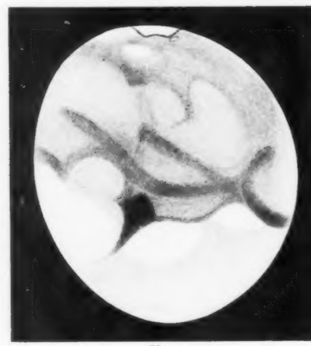
S



N

FIG. 1. July 30, 1894, 16h. 16m.-17h. 35m. M. M. T.
Power 420 to 630. Seeing 5-9. 1″ = 3.6 mm.

S



N

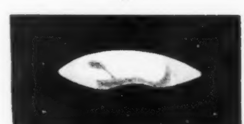
FIG. 2. Aug. 16, 1894, 17 h 35m.-18h. 20m. M. M. T.
Power 420. Seeing 9. 1″ = 3.1 mm.

S

13.24 13.30 16.15 16.23 16.56 17.30
N

FIG. 3. A series of unusually marked elevations and depressions occurring upon the terminator. All are drawn as accurately as possible to scale 1″ = 3.0 mm. Aug. 24, 1894. Seeing 3-5.

S



N

FIG. 4. July 1, 1894. 16h. 10m.-16h. 20m.
Power 420. Seeing 6-7. 1″ = 4.3 mm.

DRAWINGS OF MARS

BY WILLIAM H. PICKERING, LOWELL OBSERVATORY, FLAGSTAFF, A. T.

Scale $\frac{1}{160,000,000}$ or 1mm. = 160 km.

Astronomy and Astro-Physics, No. 128.

MARS.*

PERCIVAL LOWELL.

During the past three months Mars has been observed here every night with but few exceptions; and although it is still (Sept. 10th) a month and a half to opposition the results already obtained are very encouraging, amply confirming the importance of choosing as good air as possible for an Observatory site.

In this preliminary account of some of them I may with a certain propriety begin, so to speak, at the flood, inasmuch as the prediction which I ventured to make in my last paper with regard to the Martain vast spring freshet has already been fulfilled—although whether it be a surface freshet or an aerial one still remains in a degree doubtful. But the fact that in the planet's southern hemisphere at this season (from two months after the vernal equinox to the summer solstice) a wholesale transference of water takes place from the pole to the equator, is practically beyond question. Whether what we see be the water itself or only the effects of it is more uncertain.

On referring back to my previous paper it will be seen how large an area the dark regions then occupied and how conspicuous by their absence were those singular, tilted peninsulas that are so generally represented connecting the continents with the islands to the south. At that time one continuous belt of bluish-green stretched unbroken from the Hour-glass Sea to the columns of Hercules or rather to where this pass should have been, for it was not visible. Now the continuity is cut. Hesperia has reappeared as will be seen from one of my drawings (Plate XXV, Fig. 1) as also in the other plates by Professor Pickering and Mr. Douglass and it has done this in just the way we should expect it to show were it land drying off by a sinking of the general water level. For it will be noticed that a strait still severs it from the coast. Simultaneously, the region formerly occupied by the polar sea and the region to the north of it from having been blue, has now become for the most part reddish yellow. That this reappearance of Hesperia and change of color of the regions farther south is not due to increasing distinctness of vision consequent upon the nearer approach of the two planets is evident at a glance from this drawing and the earlier one. Had Hesperia been then of anything like the brightness it is now it could not have been invisible. Furthermore Eridania is at present one of the brightest parts of the disk not only as it comes round into

* Communicated by the author.

view but in mid-career across. Last and not least in significance the polar sea has shrunk to a thin line in keeping with the diminished size of the polar cap itself. All this water has gone somewhere.

What may be the condition of these seemingly amphibious lands, whether they be marsh chiefly water at one time and dry land at another; or whether their dark color be due to vegetation which sprouted under the action of the water, and then died when it withdrew, is a moot point. My own opinion is that it is half and half; that the transference of the water is chiefly a surface one and that the layer of water is almost everywhere so shallow as to be soon drained off. My reasons for believing the aqueous circulation to be a surface one are many. In the first place with the exception of certain peculiar appearances near the south pole, there is no evidence of anything like clouds or mist observable upon the planet, nor has there been since the observations began. On the contrary all parts of the surface seem to be revealed unveiled. For an aerial circulation the only supposition that will, in any sense, hold water is that of a heavy, nightly dew advanced by Professor Pickering. There are strong reasons in the probable constitution of the Martian atmosphere for believing this possible. But in view of certain facts connected with the canals to be specified later the dew theory seems to me improbable.

As to how much of the dark areas are water and how much vegetation there is as yet no evidence to decide. Professor Pickering has made some ingenious polariscopic observations to this end but the difficulties inherent in the process are such as to preclude definite answer as yet. At first the polar sea seemed to show evidence of polarization, confirming what we knew before of its watery character. Later the lakes, polar sea and dark areas alike revealed no trace of it. Inasmuch then as we have every reason to suspect the polar sea, at least, to be water, we are left in doubt as to the adequacy of the instrumental means to detect at present such minute phenomena. Possibly when the planet gets nearer we may learn something more definite.

Since my last paper irregularities have been detected here in the Martian terminator. These fall for the most part under two heads, of which one is practically new. It consists of certain polygonal flattenings first observed on June 30th by Mr. Douglass. Since then these irregularities have become so conspicuous that it is now difficult not to see one of them in the course of an hour's observation. Sometimes they show as simple slices shaved

off the terminator, a paring of the planet's surface; sometimes they appear bordered by enclosing projections. They range from twenty to forty degrees wide. But the suggestive thing about them is that they show almost invariably upon that part of the terminator where the darker of the dark regions is then passing out of sight. (Plate XXV, Fig. 1.)

At first blush it might seem as if the observed appearance were directly due to the darker areas lying at a lower level than the rest of the surface. But the connection is not so simply direct. For were it due to a zone of low level lying between zones of higher altitude, it could be evident to us only as a limb effect in this case still further diminished by the cosine of the phase angle, a quantity far too small to be observable.

Nor will variations in slope explain the phenomenon. For to have an area show as a depression on account of its slope either the areas on both sides of it must be rising in altitude or the area itself must be falling in height, and this state of affairs could not go on forever unless the surface were an impossible spiral. So that the persistency of this flattening is thus unaccounted for.

One supposition remains due to my friend Professor Story: that these dark areas have smooth surfaces such as water would have. In this case the reflection from them, by which alone they would be preceptible to us, would diminish much more rapidly from the centre to the side than would be the case with regions having rough surfaces such as deserts. It may be added that though a rough surface properly constructed might obliterate itself by its own shadows, this could not happen to either a desert or a forest or a grass grown plain.

Such uniform flattening is strikingly the rule with the darker parts of the dark areas. Examples of it appear throughout the drawings. Certain exceptions to it are also suggestive. In Plate XXIII, Fig. 3, Professor Pickering has a remarkable series of them. They are drawn as they appeared; their effect increased, it must be remembered, by irradiation. These may perhaps be places water and vegetation meet.

The color of these dark areas is likewise suggestive. It is to my eye everywhere a bluish-green, a sort of robin's-egg blue. The purplish brown tint seen here in spots and which I have myself seen at the north-eastern extremity of the Cimmerian Sea, I believe to be due to poor seeing as this spot shortly after under better definition appeared the usual blue-green. Its temporary tint was probably due to a mixing up of its light in poor seeing with that of the reddish orange continental coast.

The second kind of irregularities are projections or small notches such as are visible upon the lunar terminator; only that the Martian ones are much less pronounced. They are probably due to mountains which seem to be of no great height. The first of these was observed by Mr. Douglass on June 30th. An especially prominent one he noted on August 19th. It consisted of a projection flanked by a long shadow cutting into the planet obliquely. He measured the shadow's length at .35″. Taking the obliquity into account this seems to imply a range the length of whose projection would be about .2″. It is difficult to say how much of this is due to irradiation; especially as each observer differs. The best tests I have been able to make give a probable average of about five-sevenths of a tenth of a second of arc with the power then applied, about 640. Calling the terminal projection of this range therefore .13″ we have for its height about 3,700 feet. But the smallness of the quantity measured and the uncertainty of the factor of irradiation renders the result largely indefinite.

A consequence of the slope on the effect of these mountains is interesting. For an elevation need not appear as such. What would show as a projection on the nether side of the terminator would appear as a depression on the hither one.

Interesting plateaus were observed on two occasions by Professor Pickering, one of which figures in his drawing (Plate XXIII, Fig. 2). The other, a quite similar one, of which after seeing it I computed the position, turns out to be in an interesting place; for it lies in Phætontis not far from the columns of Hercules which thus seem to have been most appositely named. Both plateaus rise abruptly, are surprisingly level on top, and stand at about the same height, a height which from the reduced measurements does not probably exceed 2,600 feet.

On Mars the second kind of irregularity is less common than the first and the elevations indicated apparently never what we should call high. We may therefore conclude that the Martian surface is, as compared with our terrestrial one, relatively flat.

Certain whitish patches have been observed on the planet, first by Professor Pickering on August 16 and subsequently several times by both Professor Pickering, Mr. Douglass and myself. Professor Pickering calls them clouds, a nomenclature in which I do not wholly concur. To my eye appearances he thus designates are of two kinds. The one, certain whitish, flocular patches not far from the pole, may possibly be cloud; they certainly present a peculiar aspect, not like snow nor yet like *terra firma*. No motion, however, has been seen in them. The others

PLATE XXIV.

S

FIG. 1. June 13, 1894, 16h. 15m.
Long. 165°. Diam. 9″.6. Seeing 7 to 8.

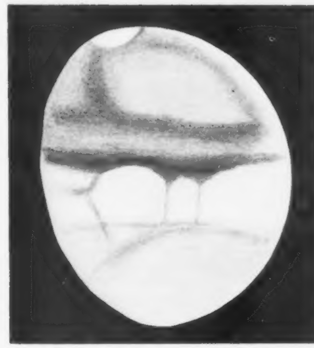


FIG. 2. July 22, 1894, 17h. 15m.
Long. 154°. Diam. 12″.62. Seeing 4 to 7.

FIG. 3. July 29, 1894, 16h. 07m.
Long. 70°. Diam. 13″.7. Seeing 4 to 10.

FIG. 4. Aug. 11, 1894, 18h. 22m.
Long. 338°. Diam. 14″.8. Seeing 4 to 8.

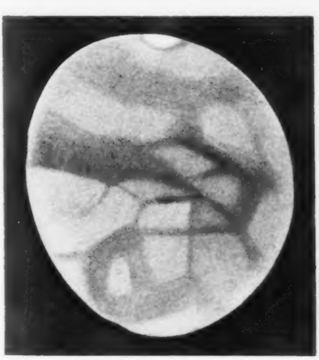


FIG. 5. Aug. 21, 1894, 16h. 56m.
Long. 223°. Diam. 16″.0. Seeing 6 to 8.

N

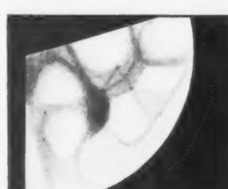


FIG. 6. Aug. 14, 1894, 16h. 02m.
Seeing 6 to 8.

FIG. 7. Aug. 19, 1894, 13h. 21m.
Near Mare Sirenum.

DRAWINGS OF MARS

BY A. E. DOUGLASS, LOWELL OBSERVATORY, FLAGSTAFF, A. T.

Scale $\frac{1}{160,000,000}$ or 1 mm. = 160 km.

Astronomy and Astro-Physics No. 128.

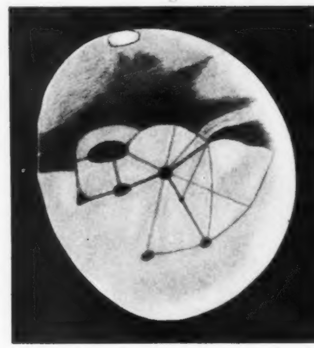
PLATE XXV.

S

N

FIG. 1. Aug. 20, 1894. 15h. 25m.
Long: Schia. 225°; Marth 210; Power 370.

S



N

FIG. 2. Aug. 29, 1894. 13h. 40m.
Long: Schia. 100°; Marth 100°; Power 420.

S

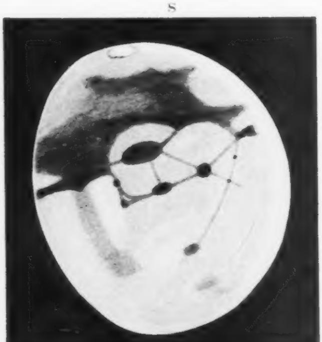


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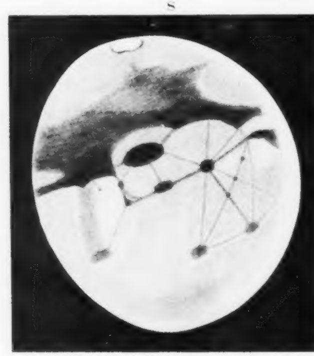
FIG. 3. Aug. 31, 1894. 13h. 45m.
Long: Schia. 88°, Marth 83: Power 420.

S



N

FIG. 4. Lake of the Sun. Long: 90°.
Aug. 27 - Sept. 2, 1894.

DRAWINGS OF MARS.

BY PERCIVAL LOWELL, LOWELL OBSERVATORY, FLAGSTAFF, A. T.

Scale $\frac{1}{160,000,000}$ or 1 mm. = 160 km.

Astronomy and Astro-Physics No. 128.

are merely certain bright spots on the general surface of the planet. These to me are not whitish but yellowish and will probably do very well for the more arid, dried up tops of the land. They likewise do not move, and furthermore show always the same appearance day after day as regularly as their regions come round. Many of them were equally conspicuous at previous oppositions and have been chronicled by various observers. Their contours are neither shifty nor indistinct but as sharp-cut as those of any other region. Elysium, Eridania and the islands to the south of it, the western part of Memnonia and the land of Ophir are of this category. Of these Elysium is perhaps the most vivid, and Eridania the least so. If conspicuous aridity be the cause of this their brilliancy, Elysium would seem antipodally named.

Most suggestive of all Martian phenomena are the canals. Were they more generally observable, the world would have been spared much scepticism and more theory. They may, of course, not be artificial but observations here indicate that they are; as will I think, appear from the drawings. For it is one thing to see two or three canals and quite another to have the planet's surface mapped with them upon a most elaborate system of triangulation.

In the first place they are at this season bluish-green, of the same color as the seas into which the longer ones all eventually debouch. In the next place they are almost without exception geodetically straight, supernaturally so, and this in spite of their leading in every possible direction. Then they are of apparently nearly uniform width throughout their length. What they are is another matter. Their mere aspect, however, is enough to cause all theories about glaciation fissures or surface cracks to die an instant and natural death.

But it is their singular arrangement that is most suggestively impressive. They have every appearance of having been laid out on a definite and highly economic plan (Plate XXV, Figs. 2, 3, 4). They cut up the surface of the planet into a net-work of triangles instantly suggestive of design. What is more at each of the junctions there is apparently a dark spot. This feature seems to be invariable as on closer approach, junction after junction turns out to have one. The larger of these appear on Schiaparelli's chart as lakes. But there would seem to be a small infinity of smaller ones. A short half-hundred of them were seen at Arequipa in 1892 and others have recently been detected here. For example an important new canal, which runs from the western end of the sea of the Sirens to Ceraunius and which in view of its point of

departure I am tempted to call the Ulysses, passes through three of these small dark spots on the way, one at each junction. One of these was seen at Arequipa and elsewhere in 1892; the other two are new—by which I mean that they have not been seen, not that they did not exist, before. The region of the Lake of the Sun is especially fertile in canals. In Plate XXV, Fig. 4, will be seen 31 of them counting each line between junctions as a separate canal. Of these 17 are among those in Schiaparelli's chart, while 14 are not. Of the 12 lakes in the figure, 5 are not down on his chart. This is not in general due to change.

Changes, however, there apparently have been after all due discount has been made for difference of observations and of drawing. First and foremost the Golden Chersonese has vanished; the land of Ophir now forms the continental coast-line. Secondly, Icaria has entirely altered in contour, resembling now an open fan about the Phœnix lake for pivot. Phætontis has shrunk to one-third of its former width—as represented in Schiaparelli's chart. Eosphoros no longer enters Phœnix lake at the point opposite Pyriphlegethon but farther to the west. But the strangest transformation of all is that of the Phasis, which has apparently obligingly become two (not geminated in the technical sense) to suit both the old and the new state of things. There is now a canal running in the same direction as the old Phasis but not to the southern end of Phætontis and there is another one running to the southern end of Phætontis, but not in the same direction as heretofore. This attempt to carry out two apparently important ends by self-multiplication is not a common characteristic of inanimate nature—a point worth consideration.

To my eye the Araxes is perfectly straight although to both Professor Pickering and Mr. Douglass it appears curved. But owing to my having observed pretty unintermittently the region about the lake of the Sun at this last presentation neither of these observers has yet had the chance to see the new upper Phasis, debouching at the southern end of Phætontis and it is, perhaps, to this canal confused with the Araxes that the observed curved effect is due.

In an interesting drawing (Plate XXIV Fig. 6), Mr. Douglass gives a couple of canals cutting each other at right angles upon Oenotria just as that island began to show differentiated from the dark area. This is a second rectangular instance of canal arrangement for an island too small for more general subdivision; Hellas being the first.

Just as this account leaves my hands, Mr. Douglass has seen (Sept. 10th) Deimos and Phobos at elongation.

LOWELL OBSERVATORY, Sept. 11th.

Astro-Physics.

THE SPECTRUM OF α HERCULIS.*

T. E. ESPIN.

The spectrum has been frequently observed, and is given by Dunér in his memoir *Sur les étoiles á spectres de la troisième classe*, as the second form of a third-type star. Secchi observed it in detail with the following results (Birmingham: *Red Stars*, First edition, p. 321):

"Magnificent columns, as usual; columns all resolved into the finest lines; vivacity of light extraordinary, though the star appears scarcely 3 mag. to the naked eye; 10th, 15th July, 1868 *Mem. II.* Singular and magnificent object, appearing like a series of convex columns illuminated by the Sun. The lines that separate the columns are profoundly black, and wonderfully distinct. The greatest light appears towards one side of the interval between the black lines, and there is a gradation of shade as in columns represented in a drawing; measuring easy; with the compound spectroscope magnesium line determined; sodium probable but not certain; only feeble indications of decomposition with simple spectroscope and power 400; but decided decomposition into fine lines, with compound spectroscope and power of 600—*Catalogs*, etc., 1867. June, 1870: No decomposition with objective prism; tried cylindrical lens with no other result; impossible to believe it a metallic spectrum.—*Sugli Spettri*, etc., Marzo, 1872. Dunér says "the bands 1–10 are excessively large and black." He also made measures of the bands, and a drawing of the spectrum is given. In coming across the star in 1894, June 29, I was so much struck by the additional detail that I made some notes on the subject. The instrument used was the 17¼-inch Calver equatorial. The spectroscope consisted of a compound prism 3 inches long before the eyepiece, without any slit or cylindrical lens. A second prism, the length of which is $4\frac{1}{10}$ in., was also used. The prisms can be made to slide away from the eye piece, thus increasing the dispersion. Powers of 200 and 500 were used.

The following are my results:

1894, June 29. Band 4 is fainter than 5, and 8 deeper than 7. Band 7 is divided by brighter parts, not lines. Band 8 is double as in Mira. Band 9 is triple, and the space from 10 to 9 is occu-

* Communicated by the author.

pied by bright and dark bands. Band 1 is divided into two; bands 2 and 3 seem resolved into lines. Band 1 is deeper than 2 and 3. Star pale orange. The results were so interesting that I made preparations for drawing the spectrum at the first opportunity. For this purpose the position of Dunér's bands were carefully laid down on paper. On the next night, June 30, the accompanying drawing was made. The following are my observations on that night:

Band 1 is double; 2 and 3 are very dark; 5 is, I believe, double; 6 and 7 are separated by a bright band; between 5 and 6 there are certainly two bright bands, and probably more. The space seems occupied with bright bands and dark shadings. On the more refrangible side of 7 is a faint dark line. Band 8 is divided into two. The second band is almost as strong as the first. The bright part between the two is not very conspicuous. At band 9 there are two bright parts and probably more. At band 10 are two bright flutings of about equal intensity, and the whole space between 9 and 10 seems full of bright and dark spaces, probably at least six. Beyond this the spectrum is not well seen but seems to be broken up into numerous bright and dark bands.

The drawing, although rough, will serve to indicate the general appearance of the spectrum. The additional detail was laid down by estimation as I have no apparatus for making direct measurements. It would, however, appear that the spectrum of α Herculis and probably of many of the stars of Type III is far more complex than has been generally supposed.

TOWLAW, Darlington, 1894, Aug. 4th.

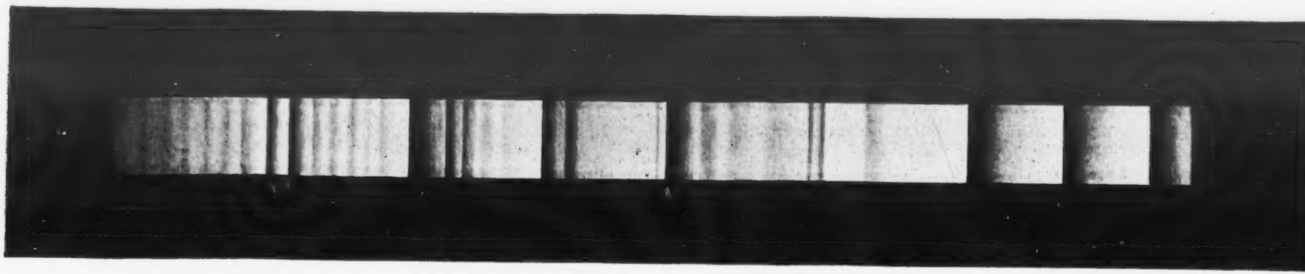
STELLAR PHOTOMETRY.*

HENRY M. PARKHURST.

Properly speaking, the art of measuring light has not been discovered. We cannot subtract from the given light a determinate amount and subject the remainder to independent determination. What is called measuring light consists in either reducing the given light in a known proportion, making it apparently equal to another standard light, or in reducing the given light in a known proportion, making it too faint to be perceptible. Both these methods are subject to so many disturbing causes that it

* Read at the Brooklyn Meeting of the American Association for the Advancement of Science. Communicated by the author.

PLATE XXVI.



10 9 8 7 6 5 4 3 2 1

THE SPECTRUM OF α HERCULIS. (1894, JUNE 30.)

ASTRONOMY AND ASTRO-PHYSICS, No. 138.

is not surprising that many astronomers are inclined to adhere to the ancient method of estimation, by following which Argelander in the northern hemisphere, Gould in the southern hemisphere, and Schönfeld in the intermediate region, have furnished us standard magnitudes for all the lucid stars, and all the brighter telescopic stars, of marvelous consistency. A series of twenty standards, half a magnitude apart, being firmly impressed upon the mind, the observer made his comparison of each star with the corresponding standard, with such interpolation of brightness as was practicable; and those who use their catalogues have many hundreds of thousands of stars to assist them in adapting their own estimations to the same system.

In a perfect photometry, each magnitude corresponds to a certain proportion of the light of the next brighter magnitude. A system in which each magnitude has a certain proportion of the light of the first magnitude, as in the astrometric scale of Sir John Herschel, or a corresponding system in which the first magnitude should be excessively faint, and the brighter stars represented by magnitudes of larger numbers, vary too much from the received scale and are too cumbrous for practical use. Sensation varies in geometric proporton; and a photometric scale founded upon geometric progression has many advantages. One of these is that it reduces the form of each quadrant of the light-curve of a regularly changing variable, nearly to a parabolic curve. But this is a subsidiary matter, the magnitudes accurately fixed in geometric progression being readily reduced to light ratios of equal precision. So far as precision is concerned, the ratio of the magnitude is unimportant. Pogson's ratio of 2½, or more accurately the number corresponding to the logarithm [0.4000] has been extensively used and is remarkably convenient. If a different ratio should be adopted by any observer, his results would only need to be multiplied by a constant to reduce them to Pogson's ratio, adding another constant to allow for difference of standard. Pogson's ratio has the merit of corresponding well with the scales or rather the scale of Argelander, Gould and Schönfeld, down to the 8th magnitude. Below this point there is a divergence in the estimation of different observers, increasing with fainter magnitudes, until they are entirely discordant. A change of ratio would not remedy this, while it would destroy the accordance with the brighter stars.

The fundamental question then is, Shall the photometric scale be fixed by unassisted estimation, or shall photometric apparatus be employed, to vary the light of the observed star, as a

means of assisting and correcting the estimation? Dr. Mendenhall, at a meeting of this Section some years ago, gave us an excellent illustration of the advantage of correcting estimation, even by means crude in the extreme. One of the earlier problems in geometry was the determination of π, the ratio of the circumference to the diameter. It can be seen at a glance that π must be greater than 2 and less than 4; but estimation would not enable us to say whether it is greater or less than 3; still less to say how much greater than 3 it is. The estimation of a thousand or a million people, especially if they attempted to agree, would not be materially better than that of one. Dividing the sum of the estimations by their number would not tend to increased accuracy. But by "tossing a stick" over a grating, a method in which each individual trial was far less accurate than rough estimation, every additional toss adds to the accuracy of the determined ratio. Tossing it several thousand times he obtained a good approximation to the value of π; and if the stick had been a mathematical line, and the grating of mathematical lines accurately placed, and if a microscope had been used in doubtful cases, it is probable that millions of trials would have carried the approximation much further. He carried it far enough to ascertain that he had a personal bias in his estimations which finally became appreciable. Now in the mode of dividing the photometric scale by unassisted estimation, the first division creates a personal bias in favor of its repetition. Having fixed upon a star of the 1st magnitude, and another of the 9th, the astronomer desires to fix the 5th magnitude midway between them. It would be superhuman to hit upon it exactly. If the first assumed 5th magnitude star is too faint, the tendency will be to make the next also too faint; and if there is a personal bias that way, the more trials are made the more definitely that standard will be settled at a point fainter than it should be. The 3d magnitude is estimated midway between the 1st and the erroneous 5th. Again an error of bias is introduced. And so of every other standard, there will be a bias. Another astronomer, using Argelander's stars, attempts to learn his scale; and even if his own bias should be in the opposite direction, he would assume it to be from his lack of experience, and would train his judgment to adopt Argelander's scale. As a final result the scale would be, as it undoubtedly is, consistent with itself, and yet there might be a large error in the uniformity of the scale. That is, the ratio of light might be different in different parts of the scale; and there would be no means of detecting it.

Suppose then we adopt photometric apparatus like the meridian photometer, in which the observed star is compared with the Pole-star, and the instrument gives the proportion of the light of the two stars when equality is apparently reached. We have here room for as much bias as before, in each individual observation. One observer may habitually make the right hand star the brighter, and another the fainter, in obtaining equality. But each individual would have an average bias, which would become more and more uniform with practice; and if the stars B, C, D, E, &c., were compared under precisely similar conditions with A, whatever bias there might be in the original comparisons, that would be eliminated when we compare the stars B, C, D, E, &c., with each other.

So in observing with the deflector, my own pet device, where the stars are reduced to invisibility with uniform illumination; if different stars are reduced to invisibility under precisely similar circumstances, whatever the point of invisibility may be, and however it may vary between one evening and another, the stars observed at the same time are free from personal bias, when compared with each other.

Other modes of assisting the judgment in like manner, if they have no inherent defect, producing systematic error, will give results continually approximating to perfection. The underlying question is whether systematic errors are guarded against; for a systematic error in a photometric instrument is as liable to mislead as a bias in estimation.

One source of systematic error in the use of the early method of diminishing apertures, and also in the use of the wedge, is the difference of illumination. As I have pointed out, (*H. C. O. Annals*, Vol. XXIX), a very large error results in the method of diminishing apertures, especially if a low magnifying power is employed, from the greater darkness of the field in the extinction of bright stars. So in the use of the wedge, the background, where the bright stars are extinguished, is much darker than where the faint stars are extinguished, and this is only partially compensated in determining the value of the wedge. This particular error does not affect observations with the meridian photometer or with the deflector. It has been cynically remarked of the Harvard observers, that their apparatus permitted an endless variety of errors, and that they had utilized them all. Perhaps the same may be said of my method. But whatever errors there may be in any method, so long as they are not systematic errors, they will tend to counterbalance each other in repeated observations.

In my judgment the most important source of error in photometric observations is from the irregularity of the sky. The individual errors may be very large, but there is no tendency to systematic error from this source. We have a remarkable proof of the existence of this same error in Argelander's estimations, where whole zones were affected. Proctor called attention to the dark streaks following circles of declination, in his projection of the Durchmusterung stars, and remarked that it was impossible that they should actually exist in the heavens. The explanation is easy. An average variation of .1 mag. in the stars observed in any particular zone, would cause so many of the fainter stars to disappear as to account for the zones so conspicuous. Certain zones were observed on one or two evenings when the sky was unusually clear, and the adjoining zones on evenings when the sky was less transparent; the effect being that those zones observed on the clearer nights, even if also observed once or twice on average nights, contain many more stars of the lower limit than the adjoining zones. The same cause of error exists in the Harvard photometric zones. There are very many of these stars which were only observed twice, and if the sky near the pole happened to be either more or less clear than in the observed zone, the whole of the stars were affected with the difference. I am satisfied that there is in many places a discrepancy of half a magnitude or more in the mean scale of magnitudes in the Harvard Zone Catalogue, resulting from this cause.

My own observations have been purely differential, the comparison stars being almost invariably so close that no allowance need to be made for difference of obscuration. But this very cir cumstance has affected them to a great extent with the local errors of the Harvard Zones, from which the standards were taken. There have been two checks. I have taken pains, especially where an error was suspected, to compare with other zones 5° away, so as to divide the error. And wherever groups of stars were compared with asteroids, those asteroids tended to reduce errors of standard.

Another source of local errors in the Harvard Zones, is to be found in the correction for atmospheric obscuration. In my photometric observations of 1869, when I independently discovered the existence and general law of the atmospheric obscuration, I ascertained that it varied on different evenings, sometimes being at least three times the average. In the Harvard Zones, the atmospheric obscuration has been assumed to be always of its average value. Consequently, on any evening when it was twice

its average value, the whole zone of stars observed would have a correction applied only half as great as was necessary, and the whole zone of stars would be affected with a constant error equal to the whole amount of the obscuration correction applied.

A fruitful source of error in observing with the meridian photometer has been the misidentification of stars. I have found many apparent instances of this in the ordinary work of the meridian photometer, where the estimated magnitude of the star was known in advance and used as a guide to assist in the identification. Chandler has found that in the observation of variable stars, where the brightness of the star was not known in advance, the errors which I attribute to misidentification were so great as to destroy or nearly destroy the value of the observations. It is not necessary for my purpose that I should question this conclusion. If it is admitted as well-founded, it only shows that in the two large catalogues, one supplementary table occupying nine pages should be discarded. When an error of misidentification occurs, in observation with the meridian photometer, it affects only the one star misidentified. The neighboring stars are absolutely unaffected. My own observations with the deflector are almost absolutely free from this danger. Each star is identified by the configuration of the group, every time it is measured. When I observe a series of ten stars, obtaining my standard from meridian photometer stars, if there has been an error of a magnitude from the misidentification of one of those stars used as standards, my resulting standard will be only .1 mag. in error, the misidentified star coming out .9 mag. different from the standard. This is a sufficient notice to reject that star and avoid the whole error, although it is only recently that I have taken the responsibility of doing this.

Since the completion of my Catalogue of Comparison Stars, I have adopted the plan of using only my own results after having obtained the general standard from the average of a sufficient number of meridian photometer stars: so that my new catalogue now forming, is free from the influence of errors of misidentification, excepting so far as they may have affected the average standard in the neighborhood. Comparison of these results in my new catalogue with the corresponding magnitudes in the meridian photometer observations, will identify the stars which have varied. In some of these cases I have no doubt that the variation has been in the stars themselves: in other cases there may have been misidentification or other error. In either case the stars are selected which are doubtful, and the rest of the stars

may be accepted as reliable. It may be that some of my own results are affected with error: I do not claim to be infallible. I only claim that my new results are so far independent of Pickering's that whenever they agree each confirms the other, and when they disagree each throws doubt upon the other or upon the constancy of the star. I do not suppose that either Cambridge or Brooklyn was selected for photometric observations in consequence of the clearness of its atmosphere. I am fully satisfied that the probable error of my own observations consists chiefly of the variations of the sky; and I have no doubt this is equally true of the Harvard observations. This opinion is confirmed by learning that Müller and Kempf, at Potsdam, claim for their photometric observations a probable error much less than I had ascertained resulted in my own observations from variations of the sky alone, after eliminating errors purely of observation. I have found that on especially clear evenings my probable error does not exceed that claimed by Müller and Kempf, while on the average it equals that of the meridian photometer. But this is not a systematic error; it only increases the number of observations necessary to obtain accuracy, and increases the uncertainty of the individual observations of variable stars, where there is nothing to counterbalance it. This confirms the conclusion I arrived at long ago, that under the atmospheric conditions, it is better to multiply observations on different evenings, than to seek to increase the accuracy of individual observations.

Since the completion of my Catalogue of Comparison Stars [*H. C. O. Annals*, vol. XXIX], I have started anew, cutting myself aloof from all errors of individual stars in the Harvard catalogues, or in my own earlier observations. As yet I have only to a very slight extent eliminated the errors affecting zones of stars. But a comparison of my results with each other, and with the meridian photometer results, and with the results of Müller and Kempf at Potsdam, will already test the extent of the liability to errors of misidentification or other errors affecting individual stars. My observations, so far as reduced and transferred, for about 16 months following Jan. 1, 1893, include 936 stars, each observed from once to more than 50 times, (excluding, of course, known and suspected variables), contained in 110 groups. From these I have selected such as were suitable for this investigation, from having been observed with the meridian photometer, and with the deflector often enough for comparison, upon the basis of two meridian photometer observations or eight extinctions for each summarized observation. This gave me for investigation

191 stars, contained in 63 groups. The mean difference in comparing meridian photometer observations with each other, was .13 mag.; the mean difference in comparing deflector observations with each other, was .14 mag. Although the last decimal may not be precise, from the inclusion among the summarized observations in both cases of stars observed more than the standard number of times, this demonstrates that the errors with either instrument are so exceptional that they do not seriously affect the catalogue as a whole. But when I compared the meridian photometer observations with my own, the mean difference was .22 mag., or at least 60 per cent greater than in comparing either set with each other. The difference, amounting to more than the mean error of either catalogue, I attribute to two causes: First, the change of my values in certain localities from equalizing the obscuration errors before referred to; and second, changes in the stars themselves. The progress of equalization has necessarily been so slow that I am disposed to attribute the principal part of this special discrepancy to changes in the stars. Professor Harkness, in his address last night, spoke of the advantage of employing different methods, from the fact that the constant errors of one become accidental errors in another. As soon as I shall have sufficient material, I intend to investigate whether there may not be systematic errors in one or the other of these two photometric methods, since such errors would tend to produce just such a discrepancy as is here manifested. In 4 per cent of the stars rejected by my criterion as fluctuating, this variation may amount to half a magnitude or more. It will require a more careful examination than I have considered my present materials to warrant, to determine the minor actual changes of the stars generally. It is enough for my present purpose if I have shown from independent observation, that the criticisms of Dr. Chandler are not applicable to the main body of the observations with the meridian photometer, but chiefly relate to the attempt to use the meridian photometer in the observation of variable stars, where there is no guide in the identification of the observed star in the knowledge of its approximate brightness.

In the recent volume of photometric observations by Müller and Kempf at Potsdam, there is a repeated expression of the opinion that the Harvard observations were conducted with too great haste, and that this led, not only to general inaccuracy but also to the misidentifications already spoken of. Upon that question I wish to make some suggestions with regard to the advantages for which the Harvard observers hazarded inaccuracy and even occasional errors.

An observer with a meridian circle in a first class Observatory takes up the Durchmusterung and at once exclaims, "This work has been done with excessive haste; the positions are several seconds of arc in error; sometimes neighboring stars are confounded, two being observed as one; for an astronomer who wants to obtain accurate positions of comparison stars for a comet or asteroid, your work is valueless." The reply is that it is better to have a comparison star close by, whose position is roughly known, than one several degrees away which is exact; for the comparisons can be made and the position of the comparison star subsequently ascertained as accurately as need be. In consequence of the irregularity of the sky, it is necessary that most photometric observations should be differential, and for the same reason it is important that the standards should be close at hand. Under these circumstances it was far more important at the time the Zone Catalogue was preparing, that it should contain many stars than that it should contain a few stars accurately observed. If three or more consistent zone stars are combined to form a standard, it is easy to weed out errors, and inaccuracies will tend to counterbalance each other. Pickering has done for photometry what Argelander, Gould and Schönfeld have done for the determination of positions, a work to be supplemented only by the new photographic survey; while it may be that Müller and Kempf stand more nearly in the relation of the meridian circle observers, to determine with accuracy selected stars.

THE MAGNESIUM SPECTRUM AS AN INDEX TO THE TEMPERATURE OF THE STARS.

JAMES E. KEELER.

In a paper recently published in the *Sitzungsberichte* of the Berlin Academy of Sciences*, Professor Scheiner calls attention to the opposite behavior, under varying conditions of temperature, of two lines in the spectrum of magnesium, and shows that these lines, taken together, give a means of estimating the approximate temperature of the absorptive atmospheres of the stars. The line λ 4482 is not found in the flame or arc spectrum of magnesium, but is very broad and strong in the spectrum of the spark with Leyden jar; the line λ 4352, on the other hand, is either invisible or very faint in the spark spectrum and strong in

* Translated in ASTRONOMY AND ASTRO-PHYSICS, August, 1894.

the spectrum of the electric arc. In stellar spectra similar differences in the relative intensity of these lines are found, and these differences are in harmony with the supposition that the temperature of the absorbing layer of stars of class I*a* is approximately that of the spark with Leyden jar, and the temperature of stars of class III*a* is approximately that of the electric arc, the opposed characteristics of the lines enabling us to discriminate between the effects of temperature and pressure. The method proposed by Professor Scheiner is certainly a plausible one, and it agrees well with what is known of the temperature of the stars on other grounds, but as the electric spark marks the limit of temperature which can at present be produced in the laboratory, it gives us no means of recognizing stellar temperatures which exceed that limit.

In this connection the behavior of the characteristic magnesium triplet *b*, which is below the lower limit of Professor Scheiner's photographs, is of very great interest. In a recently published paper on the spectra of the Orion Nebula and the Orion Stars,* I have referred to the circumstance that this group is absent from the spectrum of Rigel, in which the magnesium line λ 4482 is conspicuous, and it becomes of importance to ascertain the conditions on which the disappearance of the *b* group depends. So far as my own experience goes, and so far as I can gather from the published observations of others, the *b* lines are strong in the flame, arc, and spark spectrum of magnesium. The range of temperature which we can command in laboratory experiments is therefore not sufficient to decide the question; but from the appearance of these lines in star spectra, considered in relation to the lines mentioned by Scheiner, their disappearance seems to be the result of a temperature higher than any that can be obtained artificially.

Thus, the *b* lines are strong (apparently somewhat stronger than the solar lines) in Betelgeuse, Antares and other stars of class III*a*; they are of about solar strength in Capella and Arcturus, and weak in Sirius and Vega; they fail altogether† in α Cygni, in which the line λ 4482 is remarkably conspicuous, and in Rigel.

The following considerations seem to apply to these facts: neither of the lines referred to by Scheiner belongs to one of the series of characteristic magnesium triplets; the *b* lines, on the

* ASTRONOMY AND ASTRO-PHYSICS, June, 1894.

† By this I mean that they fail to appear on my photographs, which, nevertheless, show quite faint lines. In α Cygni there are several lines in the vicinity of the *b* group, but the *b* lines themselves are not represented.

other hand, belong to a series which, from analogy with the spectra of the alkalies, is called by Kayser and Runge the second subordinate series of magnesium. For reasons which are set forth in their memoir, it is probable that the molecular structure indicated by lines having the characteristics of a subordinate series cannot exist at a very high temperature, and Kayser and Runge have in this way accounted for the absence from the solar spectrum of all the sodium pairs which do not belong to the principal series containing the *D* lines. The same reasoning applies to the magnesium spectrum, the only difference being that a higher temperature is required to cause the disappearance of the series to which the *b* group belongs.

If this reasoning is correct, the aspect of the *b* lines in stellar spectra gives us an extension of the method proposed by Scheiner, and it shows that the temperature of certain stars exceeds that of the most powerful electric spark. That Rigel should be one of these stars is somewhat surprising, considering the place which it probably occupies in the scale of development, but the reversal of the D_3 line in its spectrum seems to point to the same conclusion, and without further observation it cannot be said that the high temperature assigned to this star by the proposed method is a real difficulty.—*A. N. 3245.*

ALLEGHENY OBSERVATORY, 1894, June 15.

The note printed above requires a slight correction. α Cygni is there mentioned as a star of class I*b* whose spectrum does not contain the *b* lines, although the magnesium line λ 4482 is very conspicuous. Recent photographs taken with high dispersion show that the *b* lines are present, although they are faint, and not easily recognized on account of the presence of much stronger lines in the same region. One of these lines is nearly coincident with b_3, and is probably identical with a strong line which I have described in the spectrum of Rigel.

ON SOME ATTEMPTS TO PHOTOGRAPH THE SOLAR CORONA WITHOUT AN ECLIPSE.*

GEORGE E. HALE.

PREVIOUS INVESTIGATIONS.

The search for a method to render the corona visible to the eye, or to secure its image upon a photographic plate, without the

* Read at the Brooklyn Meeting of the American Association for the Advancement of Science, Aug. 17, 1894. Communicated by the author.

aid of a total eclipse, has been prosecuted for many years. Professor Langley has observed the Sun from the slopes of Mount Etna, from Pike's Peak* and from Mount Whitney, but even when the atmospheric conditions were at their best he could see nothing of the true corona. At Mount Whitney the sky was of a deep violet blue, and absolutely cloudless, with only a slight orange tint about the horizon at sunset. "Carrying a screen in the hand between the eye and the Sun, till the eye is shaded from the direct rays, it can follow this blue up to the edge of the solar disk without finding in it any loss of this deep violet or any milkiness as it approaches the limb. It is an incomparably beautiful sky for the observer's purpose, such as I have not seen equalled in the Rocky Mountains, in Egypt, or on Mount Etna. . . . I found that I could choose a position on the north of the cliff, along whose edge the Sun was moving nearly horizontally; so that the shadow was fixed as regards the observer, and so sharp that, though I must have been over one-quarter of a mile from the portion of the cliff casting it, I could, without moving from my place, and by only a slight motion of the head, put the eye in or out of view of the Sun's north limb. The rocks were, in these circumstances, lined with a brilliant silver edge, due to diffraction. This I had anticipated, but now I saw what could not be seen by screening the Sun with a near object, that the sky really did not maintain the same violet blue up to the Sun, but that a fine coma was seen about it of about 4° diameter, nearly uniform, though it was sensibly brighter through the diameter of 1½°. Upon bringing to bear upon it an excellent portable telescope, magnifying about thirty times, I found it was composed of motes in the sunbeam, between the diffracting edge and the observer's eye. This result, if disappointing, is also interesting in another point of view, as showing that the dust-shell, which, as I have elsewhere stated, encircles our planet, exists at an altitude of at least 13,000 feet, and under favorable conditions for the purity of the atmosphere."† Other unsuccessful attempts to observe the corona without an eclipse have been made by Professor Bond in the Alps, Dr. Copeland‡ at Puno at a height of 12,940 feet, Professor Tacchini on Mount Etna, and Professor Todd on Fuji-san. Professor Wright has tried various colored media in the hope of rendering the corona visible, and Professor Harkness§ and Dr. Pupin‖ have

* *Reports on the Total Solar Eclipses of July 29, 1878 and January 11, 1880*, p. 207.

† S. P. Langley. *Report of the Mount Whitney Expedition*, p. 41.

‡ *Copernicus*, vol. III, p. 212.

§ *Bul. Phil. Soc. of Washington*, vol. X, p. 13.

‖ *Astronomy and Astro-Physics*, vol. XII, 1893, p. 362.

devised special apparatus for the same purpose. But the most serious attempt to solve the problem is that of Dr. Huggins, and this calls for more detailed consideration.

After trying in vain a variety of optical devices, including the crossed prisms afterward employed by Harkness and Deslandres, Dr. Huggins decided to avail himself of the power of the photographic plate to render visible smaller differences in brightness than are directly perceptible to the eye. By experiments in the laboratory he satisfied himself "that under suitable conditions of exposure and development a photographic plate can be made to record minute differences of illumination existing in different parts of a bright object, such as a sheet of drawing paper, which are so subtle as to be at the very limit of the power of recognition of a trained eye, and even, as it appeared to me, those which surpass that limit."* Professor Schuster's photographs of the coronal spectrum obtained at the eclipse of May 17, 1882, showed a maximum brightness between G and H, and it seemed not improbable that an exclusive use of this portion of the spectrum might make it possible to photograph the corona without an eclipse. Later a further argument in favor of the employment of this part of the spectrum was found in the results of Professor Vogel's measures of the absorption of the photospheric light at the center and limb of the Sun. It was discovered that the violet suffered much more absorption than the red, the percentage brightness at the limb compared with the center of the disk being 13 and 30 respectively. As most authorities agree that the rapid increase in absorption toward the limb would indicate that the absorbing gas rises to no great height above the photosphere, the light of the corona, which lies outside the low region of absorption, would reach the Earth without undergoing this absorption. Thus it should be relatively rich in the violet, when compared with the photospheric light which is scattered in our atmosphere.† The advantages offered by photography, especially for work in the more refrangible region of the spectrum, were thus sufficient to lead to its substitution for the visual methods previously employed.

The first experiments were made with photographic lenses, but these were soon abandoned for a Newtonian reflector of 6 inches aperture and about 3½ feet focus. The solar image was received upon a photographic plate, in front of which were placed the absorptive media. The exposing shutter, supported before the end

* *Proc. Roy. Soc.*, vol. XXXIV, 1882, p. 411.
† *Proc. Roy. Soc.*, vol. XXXIX, 1885, p. 111.

of the telescope, was of adjustable rapidity, and reduced the aperture to three inches. The telescope was not provided with a driving clock, on account of the shortness of the exposures. For absorbing media a special violet glass in the form of flat polished plates, and later a glass cell with polished sides containing a strong fresh solution of potassic permanganate, were employed. The gelatine dry plates were backed with a solution of asphaltum in benzole. The exposures ranged from that just sufficient for the Sun itself, to one so long as to reverse the photographic image out to some distance from the Sun's limb.

Between June and October, 1882, twenty plates were secured which showed forms closely resembling the corona, not merely in a general brightening near the Sun, but in the presence of distinct coronal forms and rays. The plates taken on different days with different absorptive media, and with the Sun in different parts of the field, agreed so well among themselves that they could not be attributed to any instrumental effect. The very short exposures showed only the inner corona, but with increased exposure the inner corona was lost in the outer corona, which exhibited the curved rays and rifts observed at eclipses. The corona was most easily seen, however, in those plates which were exposed long enough to photographically reverse both the photospheric and coronal images. A careful comparison of the photographs with those secured at the Egyptian eclipse of the same year showed so perfect an agreement of general features and even details as to convince both Dr. Huggins and Captain Abney that the problem had been solved.*

In his later work† Dr. Huggins did away with the second reflection of the Newtonian telescope, and by slightly inclining the speculum metal mirror caused the solar image to fall directly upon a silver chloride plate, which Captain Abney had shown to be most sensitive to light from h to a little beyond H. The absorbing media were thus considered unnecessary. A long tube with numerous diaphragms was placed in front of the telescope, and every care was taken to avoid difficulties arising from diffuse or reflected light. Photographs taken with this apparatus immediately before and after the eclipse of 1883 showed coronal forms very similar to those obtained at Caroline Island during totality, a dark rift near the north pole of the Sun being particularly noticeable in both cases.

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* *Proc. Roy. Soc.*, vol. XXXIV, 1882, pp. 411-414.

† *Report B. A. A. Science*, 1883, p. 346, also *Proc. Roy. Soc.*, vol. XXXIX, 1885, p. 113.

In 1884 a committee appointed by the Royal Society sent Mr. Ray Woods to the Riffleberg, near Zermatt, for the purpose of continuing Dr. Huggins' experiments at an elevation of 8,500 feet. In spite of the continued presence of a great aureole around the Sun, due to the presence of minute particles of matter in the higher regions of the atmosphere, Mr. Woods obtained coronal images which were very much alike on the same day, but showed variations when separated by longer periods. The use of a disk to cover the Sun's image seemed to be of no advantage. The best results were obtained on the clearest days.*

During the next two years no experiments in this direction were made in England, owing to the prevalence of whitish skies. In 1886, however, the method was put to the test at the time of the total eclipse of that year. Captain Darwin photographed the Sun on the day before the eclipse, but the coronal images obtained showed no similarity with the true coronal images photographed during totality. On the day of the eclipse exposures made during the partial phases showed a false corona, part of which was in front of the Moon. In no case did the images show the Moon eclipsing the corona. Instantaneous exposures made during totality gave no trace of the corona. Although the conditions were most unfavorable during this eclipse Captain Darwin concludes "that the result tends to show that a *practical* method of obtaining photographic records of the corona during sunlight is not likely to be obtained," at the same time remarking that he does not consider it proved that the method is impossible.†

At Cape Town Dr. Gill photographed the partial phases of the same eclipse with a Huggins coronagraph, but the results were equally disappointing. A long series of experiments made at the Cape Observatory with the coronagraph has not led to results of a definitive character.‡ Nor have the attempts in this direction of Dr. Lohse,§ Herr von Gothard‖ and M. Deslandres¶ resulted more satisfactorily.

Professor Tacchini's observations on Mount Etna in 1876** led to the establishment of the Bellini Observatory at the base of the great crater a few years later. With a Huggins coronagraph

* *Observatory*, vol. VII, 1884, p. 378.
† *Proc. Roy. Soc.*, vol. XLI, 1886, p. 470.
‡ *Report of H. M. Astronomer at the Cape of Good Hope for the period 1879, May 26, to 1889, July 21*, p. 9.
§ *Astronomische Nachrichten*, vol. CIV, 1883, p. 209.
‖ Von Konkoly's *Himmelsphotographie*, p. 229.
¶ *Comptes rendus*, Jan. 23, 1893.
** *Memorie della Societa degli Spettroscopisti Italiani*, vol. V, 1876, p. 151.

constructed by Grubb, Professor Riccò has made a large number of photographs of the Sun's surroundings. Many of these show distinct coronal forms, extending to great distances from the solar limb. During the partial eclipse of April, 1893, Professor Riccò made several exposures, but the coronal forms shown on the negatives cross the dark body of the Moon without interruption, thus demonstrating that they do not represent the true corona.* Moreover, the exposures with which these and other coronal images have been secured are much shorter than those given with similar instruments during total eclipses. It is universally admitted that the corona at a distance of a solar radius from the limb is much fainter than the Moon. In Professor Riccò's photographs, which were made with an exposure of less than half a second, the coronal forms extend to a distance of nearly a solar diameter. Using the same apparatus and equally sensitive plates we obtained no traces of the Moon (which was well past its first quarter) with an exposure of four seconds, during our recent expedition to Mount Etna. It is, therefore, highly probable that the coronal images obtained with the Huggins apparatus on Mount Etna were of terrestrial origin.

Although I have not succeeded in photographing the corona in full sunlight, I present herewith an account of my efforts in this direction, in the hope that some of the details may prove of service to others at work in the same field.

A New Method of Coronal Photography.

At the Rochester Meeting of the American Association I described my investigations in solar photography with monochromatic light. By the aid of the spectroheliograph I had succeeded in photographing the chromosphere and prominences as well as faculæ and other phenomena on the solar disk. On June 10, 1892, I made two attempts to photograph the corona with the spectroheliograph, the exposure being much longer than that required for prominences. The sky showed a considerable brightening near the Sun on the plates, but no certain indication of the corona was obtained. The possibilities of the spectroheliograph in its application to the photography of the corona were carefully studied, and in August of the same year the construction of a special instrument for this purpose was undertaken by the mechanician of the Kenwood Observatory. It was at first proposed to isolate light of any desired wave-length by means of a spectroheliograph, and thus to photograph the sky surrounding the Sun,

* *Memorie della Societa degli Spettroscopisti Italiani,* July, 1893.

employing such a region of the spectrum as experiment proved to be best adapted to show the corona on the background of the sky.* Reasoning from Professor Vogel's measures of the solar absorption, as Dr. Huggins had previously done, and also from the comparative brightness of the coronal spectrum in the violet region, it would appear that the upper part of the spectrum might be advantageously employed, except when the blueness of the sky extends to the very limb of the Sun. Professor Langley's observation at Mount Whitney, which has been quoted at length above, would make it appear probable that this latter condition is never fully realized. M. Deslandres† has recently called attention to Lord Rayleigh's comparison of the sky and solar spectrum, from which was deduced the law that the brightness of the sky spectrum (near the zenith) is inversely proportional to the fourth power of the wave-length of the region observed. With an atmosphere free from large particles of dust and vapor the law would probably hold for the light of the sky very near the Sun's limb, and in such a case the less refrangible region could be advantageously employed with the spectroheliograph. Under ordinary conditions, however, the whiteness of the sky near the Sun is so intense as to render advisable the use of the blue or violet rays, partly on account of the lack of really satisfactory photographic plates sensitive to the red or ultra-red rays.

But further reflection convinced me that the mere isolation of a particular region of the spectrum by means of the spectroheliograph would not suffice for the difficult task of photographing the corona in full sunlight, and it was soon concluded that the most promising means of attaining this end was offered by the dark lines of the solar spectrum. As the light of the sky is merely reflected sunlight, the dark lines of its spectrum indicate that light of the corresponding wave-lengths is but feebly represented in our atmosphere. If, then, a dark line (other than telluric) in the sky spectrum is set on the second slit of a spectroheliograph, and the slit narrowed sufficiently to exclude all light except that of the line, it is evident that the protection to the photographic plate will be the same‡ as that which would result from a reduction of the brightness of the whole sky to that of the line. Whether or not the coronal light will be reduced in anything like the same proportion is the next question to be considered.

The presence of reflected sunlight in the spectrum of the cor-

* *Astronomy and Astro-Physics*, 1893, p. 260.
† *Bulletin astronomique*, February, 1894.
‡ Neglecting the effect of diffuse light in the apparatus.

ona has long been recognized, but opinions as to its amount vary widely. The principal Fraunhofer lines have been seen and photographed at many eclipses, but frequently they are so faint as to be barely perceptible. After an exhaustive examination of all observations made up to 1883, Professor Hastings remarks that "the conclusion is inevitable that the proportion of true solar light in that of the corona within 5′ or 6′ of the Moon's limb is so small that all but the strongest of the Fraunhofer lines are invisible in any spectroscope which has hitherto been employed." By mixing light giving a continuous spectrum with sunlight Professor Hastings succeeded in producing a spectrum in which the principal Fraunhofer lines were of about the same intensity as in the corona when the proportion of light giving a continuous spectrum to solar light was about two or three to one.* This is direct proof that the percentage of reflected sunlight in the coronal spectrum is small. Recent eclipses furnish evidence leading to the same conclusion, for in some photographs made with a slit spectrograph the Fraunhofer lines are nearly as well seen in the sky outside the corona as in the corona itself. Thus it appears that by setting a dark line on the second slit of a spectroheliograph the brightness of the sky spectrum may be considerably reduced, while the brightness of the coronal spectrum at the same point is not seriously diminished.

In the choice of a line for this purpose it is evident that one should be selected which is (1) of solar origin, (2) of sufficient width, (3) very dark compared with the continuous spectrum in which it lies, and (4) situated in that part of the spectrum in which the contrast between the continuous spectrum of the corona and the spectrum of the sky near the Sun is the greatest possible. Of these conditions the first can of course be fulfilled. The second is of practical importance, for on account of the feebleness of the coronal light the dispersion must be low, and thus the greater part of the Fraunhofer lines will be too narrow to afford complete protection to the plate, as the second slit cannot be closed beyond a certain point for mechanical and optical reasons, and also because with smaller slit-width the exposure would be too much prolonged. The third condition is also of great importance, but as many of the darkest lines are too narrow to be successfully employed it simply remains to choose the darkest of the sufficiently wide lines. A further complication may result from the position of the line in the spectrum. It has

* *Report of the Eclipse Expedition to Caroline Island, May, 1883*, p. 116.

been pointed out that the less refrangible rays may offer peculiar advantages when the atmospheric conditions are such that the sky is blue close to the Sun's limb. But as this condition is rarely, if ever, realized the special advantages of the lower part of the spectrum may be offset by the practical difficulties peculiar to this very region. Hitherto, in spite of the successful use of such dyes as cyanine and alizarine blue for increasing red sensitiveness. no plates have been obtained which, in point of sensitiveness to red light and freedom from fogging, can compare with the action of ordinary commercial plates in the more refrangible part of the spectrum. When truly isochromatic plates have been obtained the red or ultra-red may perhaps be advantageously employed for photographic work on the corona. But on account of our incomplete knowledge of coronal radiation and atmospheric diffusion experiment may be regarded as the best guide in the choice of the most suitable region of the spectrum.

In all of my experiments at Chicago, Pike's Peak and Mount Etna I have employed the broad dark K band of the solar spectrum, for practical rather than for theoretical reasons. With the spectroheliograph used on Mount Etna this band was but $\frac{1}{250}$ of an inch wide, and in work on the corona it is not advisable to use a second slit much narrower than this. In fact, even this slit-width requires that the exposure be greatly prolonged. It will be seen, however, that this difficulty may be diminished by employing a spectroheliograph of large effective aperture. K is preferred to H because it is somewhat broader and darker, but neither of these bands is nearly as dark as many of the lines in the solar spectrum. K was chosen simply because the feeble light of the corona does not permit the employment of a dispersion sufficiently great to make the narrower dark lines wide enough to protect the plate. If the dispersion were increased enough to allow the narrow lines to be used, or, what amounts to nearly the same thing, if the second slit were made equal in width to these narrow lines, the exposure would have to be so much increased that the advantage gained by the greater blackness of the line would probably be more than offset.

It will be noticed that the procedure here recommended for photographing the corona without an eclipse is identical with that employed daily at the Kenwood Observatory in photographing the chromosphere and the solar disk. The principle of the method is, however, very different. In the case of the chromosphere photography is rendered possible by the presence in the chromospheric spectrum of the bright K line of calcium, but while this

line is frequently spoken of as belonging also to the corona, most of the evidence is opposed to this view. It is true that eclipse photographs made with a slit spectrograph show the bright H and K lines extending far out into the corona, but these same bright lines also frequently appear across the dark body of the Moon: a result of diffusion in the Earth's atmosphere of the brilliant calcium radiation of the chromosphere and prominences. Professor Hastings has shown that diffraction at the Moon's limb may also have a part in producing the extension of the reversals into the corona,* and as the result of his observations at the eclipse of January 1, 1889, Professor Keeler concludes that superposed upon the corona, "and blended in with it, is a more or less uniform-ring of light caused by diffraction. This diffracted ring is necessarily rich in edge light of the Sun, particularly light derived from the chromosphere, and to it is due the appearance of bright lines in the corona at a considerable height above the Sun."† It is needless to multiply references to the various eclipse reports in which similar conclusions are reached, and we may merely add that Captain Abney and Professor Schuster believe that if the presence of the H and K lines in the spectrum of the corona cannot always be accounted for by atmospheric diffusion or diffraction, the calcium vapor which must then give them rise is carried up into the corona by eruptive prominences, and does not exist there in the normal condition of things.‡

But even if it could be shown in the face of all this evidence that the H and K reversals properly belong to the coronal spectrum, it would still be impossible to photograph the corona without an eclipse by their means. For if we may trust the observations of Tennant and others, the lines seen in the coronal spectrum are as bright in the dark rifts as in the brilliant streamers. Thus—supposing these lines to indicate the existence of glowing vapors at a distance from the Sun's limb—a photograph taken with a bright line would show a uniform halo about the Sun, with no traces of true coronal structure. It is evident, therefore, that we must depend upon the continuous spectrum of the corona to furnish the needed light, and the advantages offered by the spectroheliograph are the choice of light of any desired wave-length, and particularly the protection afforded to the photographic plate by shielding it from all light except that coming

* *Report of the Eclipse Expedition to Caroline Island, May, 1883*, p. 121.
† *Lick Observatory Report on the Total Eclipse of January*, 1889, p. 54.
‡ See *Phil. Trans.*, vol. 180 (1889), (A) p. 328, and other eclipse reports.

through a relatively dark line in the superposed spectra of the corona and sky.*

I have already pointed out† that the same method may perhaps be employed in photographing the "white prominences," discovered by Professor Tacchini at the Egyptian eclipse in 1882, and observed subsequently at Caroline Island and Grenada in 1883 and 1886.‡ These remarkable objects give a continuous spectrum, and as the hydrogen lines are absent they cannot be seen in full sunlight observations with the spectroscope. Photographs of the spectrum of the great white prominence, which was so conspicuous an object at the 1886 eclipse, seem to show the presence of the bright H and K lines§ and this gave reason to hope that they might be successfully photographed without an eclipse with the ordinary spectroheliograph employed for prominences. Not all of the Kenwood Observatory photographs have been reduced, but on those hitherto examined, no such objects have been found. But even if the H and K reversals do not belong to the white prominences, the method recommended above for the corona may prove to be successful for them as well, on account of the comparative brightness of their continuous spectrum.

Of first importance in the design of any apparatus to be used in photographing the corona without an eclipse is the telescope with which the solar image is formed on the first slit of the spectroheliograph. M. Deslandres has suggested the use of a single lens of crown glass or quartz for this purpose, and remarks that the doubly reflected light could be stopped by a diaphragm placed near the lens.‖ If a lens were to be selected in preference to a mirror it would seem to me advisable to give to the surfaces such a curvature and such an inclination to the optical axis of the telescope that all of the doubly reflected light would be concentrated in an image some distance to one side of the direct image of

* M. Deslandres' claims to priority on the proposed use of the spectroheliograph for photographing the corona without an eclipse (*Comptes rendus* v. CXVI, p. 1184) are admittedly based on a somewhat obscure reference in a foot-note to the employment of the *bright* H and K lines for this purpose (*Comptes rendus*, v. CXIII, p. 307). My own use of the method followed naturally from my work on the prominences, and had nothing to do with the foot-note in question. Moreover, I have never advocated the use of the *bright* H and K lines, as there is no reason to suppose that they could be successfully employed for this work.

† *Sidereal Messenger*, June, 1891, and elsewhere.

‡ For a full account of Professor Tacchini's observations of the white prominences see his interesting work, *Eclissi totali di sole del 1870, 1882, 1883, 1886 e 1887* (Roma, Tipografia Eredi Botta, 1888).

§ W. H. Pickering, *Annals of Harvard College Observatory*, vol. XVIII, No. V, p. 99.

‖ *Comptes rendus*, vol. CXVI, p. 1186.

the Sun. The chromatic aberration of the single lens would be an advantage rather than a disadvantage, and the difficulties arising from the spherical aberration and the inclination of the lens to the normal plane would hardly be sufficient to so seriously injure the coronal image as to make it unfit for our present purpose. But on account of the difficulty of completely doing away with the doubly reflected sunlight from a lens, I have preferred to follow the example of Dr. Huggins, and employ mirrors of speculum metal or silver-on-glass in all of my experiments. I wish to express my admiration for Mr. Brashear's inimitable skill in making and polishing such specula, as well as my indebtedness to him for the pains he has taken to furnish me with surfaces of the highest excellence.

In each of the three instruments successively employed at Chicago, Pike's Peak and Mount Etna the mirror has been mounted in the Herschelian form, previously adopted in the Huggins coronagraph. I have not been unmindful of the dangers of diffuse light, but from the very outset every possible precaution has been taken in the way of numerous diaphragms, dead black surfaces, perfectly polished mirrors, prisms and objectives, etc. It has been found in practice that dust is one of the principal obstacles to success, and great pains have been taken to make the apparatus as nearly as possible dust-proof, and to protect as well as might be such surfaces as were necessarily exposed to the air.

Experiments at the Kenwood Observatory.

Fig. 1, Plate XXVII, shows in outline the apparatus used in the series of experiments made at the Kenwood Observatory in the spring of 1893. A is a silver-on-glass mirror of 5½ inches aperture and 48 inches focal length. At B the solar image is formed on a small metallic disk, slightly exceeding in diameter the image of the photosphere. The mixed light of the corona and sky near the Sun then passes into the spectroheliograph through a slit at B, while the direct sunlight is excluded. If this light were allowed to enter the spectroheliograph it would greatly reduce the chances of success. The diverging pencil of sky and coronal light falls at C upon a silvered glass mirror of 4 inches aperture and 24 inches focal length. As the mirror is separated from the slit by a distance equal to its own focal length, and mounted with the normal to its surface making a small angle with the axis of collimation, the parallel pencil is reflected to D, where it falls upon a crown glass prism of 30° angle, the second surface of

which is silvered. The rays are thence returned to the collimating mirror, and the prism and mirror are so adjusted that the image of the spectrum is formed on the second slit, immediately below the first slit. The K line is made to pass through the second slit, and falls upon a photographic plate, the surface of which is in the focal plane. As the first slit is moved across the coronal image by means of water pressure in a small clepsydra, the second slit is adjusted to move in the opposite direction at such a speed as to keep the K line constantly between its jaws. Fig. 1 Plate XXX illustrates the arrangement of the slits. The frames on which the slits are carried move on steel balls, and the lever connecting the slit carriages is adjustable in length. The second slit is, of course, curved to correspond with the curvature of the K line. The mirrors A and C are adjustable for inclination and focus, and the prism can be rotated by means of a tangent screw outside the box. The framework of the apparatus is made of gas pipe, and the covering of wood, painted dead black, and provided with numerous diaphragms and screens to diminish diffuse light. With the exception of the mirrors and prism, which were made by Mr. Brashear, the apparatus was constructed in the workshop of the Kenwood Observatory. When in use it was attached to the tube of the 12-inch telescope, and the excellent driving clock would keep it directed at the Sun during any desired exposure.

The use of a single mirror in place of the ordinary objectives of the collimator and observing telescope of a spectroscope was first suggested by Lippich,* and has since been reinvented by Ebert† and Wadsworth.‡ It offers important advantages in many classes of spectroscopic work, and will no doubt share in the future some of the popularity which spectroscopes with two mirrors in place of objectives in the collimator and telescope are now beginning to enjoy.

On April 8 the apparatus was attached to the telescope, and on April 13 and 14 the various adjustments were made. These included focussing the solar image on the first slit, focussing the collimating mirror and adjusting it and the prism so that the K line fell on the second slit, setting the jaws of the second slit parallel to the K line, adjusting the length of the lever so that the K line remained on the second slit during its movement across the Sun, etc. The performance of the two mirrors was satisfactory,

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* *Zeitschrift fur Instrumentenkunde*, 1884, p. 1.
† *Sitzber. physikal. med. Soc. Erlangen*, 8 July, 1889.
‡ *Phil. Mag.*, 1894.

in spite of their necessary inclination, but I was not satisfied with the arrangement and motion of the two slits, and the clepsydra was so small that on account of the large and variable element of friction the motion was not sufficiently uniform. The K line stayed on the slit fairly well, but there was a certain amount of lost motion which made it uncertain whether the slit always followed the line accurately throughout the exposure.

On April 16, the day of the total eclipse in Africa and South America, some attempts were made to photograph the corona, but the sky was far too bright to permit a sufficiently long exposure to be given. With a motion of the slits such that the half-inch solar image was crossed in eight minutes, the sky was greatly overexposed, and another photograph showed that one minute was still too long. As my calculations had indicated that we could not hope to obtain the corona in much less than 25 minutes, the results were far from encouraging. Further experiments only served to demonstrate more clearly the hopelessness of coronal photography beneath a Chicago sky, and the shortcomings of the apparatus also became more apparent with continued use. It was therefore decided to repeat the experiments with better apparatus and the more favorable atmospheric conditions to be expected on a lofty mountain peak.

Through the kindness of Professor Harrington, Chief of the Weather Bureau, I was enabled to examine the meteorological records of many elevated stations in the West, and after careful consideration of the relative advantages of these points, I decided to make an expedition to the summit of Pike's Peak at the time of the summer solstice of 1893.

The Expedition to Pike's Peak.

The apparatus constructed in our workshop for this expedition is outlined in Fig. 2, Plate XXVII, and Plate XXIX is from a photograph taken by Professor Keeler on Pike's Peak. The same mirror used in the previous experiments forms an image of the Sun on a metallic disk at B. The mixed light of the corona and sky enters the collimator by the first slit, and is rendered parallel by an achromatic objective (C), of 2 inches aperture and 20 inches focus. The dispersion piece is a Brashear 60° prism (D) of light crown glass, perfect in purity and freedom from color. The objective (E) of the telescope is exactly similar to that of the collimator. Both were made of carefully selected glass by Mr. Brashear, and castor oil is used in each objective between the lenses. The first and second slit carriages run on steel balls, and are connected with each

other and with a clepsydra by a lever system similar to that in constant use on the large spectroheliograph of the Kenwood Observatory. The second slit carriage is provided with a totally reflecting prism attached to a microscope, which can be racked into or out of the field of view for the purpose of setting the K line on the slit, and also to enable the observer to maintain it there, if necessary, during the exposure, by means of a screw arranged to move the second slit relatively to its carriage. The spectroheliograph and sheet-iron mirror-tube are supported by a skeleton iron frame, which was bolted to the declination axis of a 6-inch equatorial mounting by Grubb, for the use of which I am indebted to the kindness of Professor H. A. Howe, Director of the Chamberlin Observatory.

Professor James E. Keeler, whose experience gained at Mount Whitney and on other important expeditions rendered his advice and assistance of incalculable value, was good enough to volunteer his services, and the many difficulties encountered on the Peak were lessened in no small degree by his presence. I desire here to express to him my grateful acknowledgments.

On June 16, 1893, our party, consisting of Professor Keeler, Mrs. Hale and the writer, left Chicago on the Chicago, Rock Island and Pacific R. R., and arrived at Manitou, Colorado, on June 18. The following extracts from our note-book will sufficiently outline the events of our stay in Colorado.

June 19. Had a wooden stand made to carry the Grubb equatorial head, and arranged to leave for the Peak next morning.

June 20. Went up the Peak on the morning train, taking a trunk filled with apparatus, and the wooden stand for the telescope. L. and I returned to Manitou; K. remained at the summit, and set up the stand in front of the Weather Bureau Station, levelled it and anchored it with rocks.

June 21. Went up on the morning train, taking the Grubb mounting and a box of apparatus. As soon as we reached the summit, K. and I placed the mounting on the wooden stand, and roughly adjusted the polar axis. In the evening we put the spectroheliograph together.

Suffered considerably from headache due to the altitude.

June 22. Continued work on the adjustments of the mounting and spectroheliograph. The first time the R. A. clamp was used the steel pin broke short off, but it was found possible to use it by employing a screw driver for clamping.

L.'s severe headache continued to grow worse, and it became impossible for her to stay on the Peak. (We afterwards found

that about two-thirds of the tourists who came up the mountain on the train each morning were affected by the altitude, and during our stay we saw one or two very serious cases of mountain sickness. While not much troubled, K. and I found prolonged hard work very fatiguing, and any slight extra exertion at once increased the action of the heart). I went down with L., and in the afternoon had a rough R. A. clamp made in Colorado Springs.

JUNE 23. Left L. at Manitou, and went up on the morning train, taking with me an express box containing the telescope tube to be used with the spectroheliograph. In the afternoon fitted spectroheliograph to mounting, and prepared it for use. In the evening found that the silvered glass mirror, which was in perfect condition a few days before, was badly tarnished. Telegraphed Brashear for silvering materials.

JUNE 24. Arranged a canvas cover to protect apparatus when not in use. Commenced adjusting mirror. Snow storm came up, so went down to Manitou with K. on afternoon train.

During our stay on the Peak we found that the sky was usually cloudless in the early morning, but every day with great regularity cumulus clouds commenced to form about the Peak between nine and ten o'clock, and by eleven o'clock the sky was generally covered with them. In going up from Manitou I observed the sky near the Sun at altitudes ranging from 8,000 to 14,000 feet, and found a steady decrease in the brightness as the summit was approached. At times during the first few days the sky appeared blue nearly up to the Sun's limb, but later extensive fires in the forests to the south sent up great volumes of smoke, which spread over the sky, and increased its whiteness. Fires also appeared in other directions, and when we left for home on July 4 the Peak was encircled with fires, and the sky was very white.

JUNE 25. At Manitou House. Sky cloudy in the morning, storm in the afternoon.

JUNE 26. Went up on monrning train. When we reached the summit the sky was clouded over, and soon after hail began to fall. Storm continued with hail and snow all the afternoon, and at times wind velocity was over 70 miles per hour. Sky cleared at sunset. Improvised a dark room in the kitchen of the Weather Bureau Staton.

JUNE 27. Worked on adjustments of apparatus. Cloudy most of the afternoon.

JUNE 28. Continued work on spectroheliograph adjustments. Optically the instrument leaves nothing to be desired, but there

is some spring in the levers, and as our only source of pressure was a pail of water a few feet above the instrument on the roof of the Station, the clepsydra did not work well. As the prism is no longer at position of minimum deviation for K when the slits are out of the center of the field it was difficult to adjust levers so that this line would stay on the second slit, but at last this was accomplished. Made first exposure at 12 M. Received silvering materials from Brashear, and re-silvered mirror in the evening.

JUNE 29. Cloudy all day. K. and I walked down to Manitou in the afternoon through a heavy snow storm, with much thunder and lightning. We could hear the continual hissing of the brush discharge from the corners of the metallic roof of the Station, and from the pointed wires set in the top of every telegraph pole. The storm was local, as we emerged into bright sunshine about two miles below the summit. As seen from Manitou the Peak was covered with clouds the rest of the day.

JUNE 30. Went up with K. on morning train. Clouds came up so rapidly that we had time for but one exposure. Sky rather white. In the evening the telescope was left uncovered for photographing the Moon, but a cloud suddenly came over the mountain and every part of the instrument was drenched before it could be covered.

JULY 1. Made third exposure at 9^h 15^m, sky hazy, mirror in bad condition. Plate 4, 9^h 40^m; sky milky near Sun; tried several speeds of clepsydra. Plate 5, 10^h 20^m, sky fair; passing clouds. Plate 6, 10^h 45^m, sky good; clouds passing over Sun. Plate 7, 11^h 15^m, sky good at times, but clouds passing over Sun during exposure, as in case of Plates 5 and 6. Further work prevented by clouds.

An attempt was made to photograph the spectrum of the Moon in the evening, but a cloud covered the mountain just after the exposure commenced.

JULY 2. It was found that the heavy canvas used to cover the telescope so disturbed the adjustments by pressure on the levers that much time was required each morning to re-adjust the spectroheliograph. Made several exposures; sky very white. During last exposures swarms of insects (also seen on previous day) were seen above the mountain in the direction of the Sun. These added considerably to brightness of sky. At 11^h 15^m, when work was stopped by clouds, the sky was very carefully examined. The Sun was seen to be surrounded by a great white haze, which grew rapidly brighter near the limb.

JULY 3. Smoke from forest fires increased so rapidly that nothing more could be done. Packed up apparatus.

JULY 4. Went down to Manitou, passing very near a forest fire on the way. Left the same afternoon for Chicago.

Whatever one's confidence in the method employed in these experiments, it is not very surprising that the negatives obtained under such conditions failed to show any trace of the corona. Not only was the purity of the sky destroyed by the smoke from the forest fires, but the lack of sufficient water pressure for the clepsydra made the motion of the slits very irregular, and the variations in friction and speed caused such springing in the levers that the K line must have been frequently off the second slit. The clepsydra had been designed for use with a much higher pressure, and when this could not be obtained it failed to communicate to the slits the necessary smooth and uniform motion. There can also be no doubt that it is undesirable, with a spectroheliograph of these dimensions, to move the slits far out of the position of minimum deviation. As I have already mentioned, the spectroheliograph was extremely satisfactory from an optical point of view, and the amount of diffuse light was very small. The dust constantly blowing into the telescope tube and on to the mirror, was very troublesome, and the frequent use of a soft camel's hair brush did not suffice to keep the mirror free, as much dust accumulated during the exposure. Silver is so soft that microscopic scratches are always cut in it when polishing. On account of its comparative hardness and freedom from liability to tarnish speculum metal is to be preferred to chemically deposited silver for work on the corona. A still better form of mirror will be mentioned below.

I desire to express the thanks of our party to R. R. Cable, Esq., President of the Chicago, Rock Island and Pacific R. R., through whose courtesy we were supplied with round-trip tickets from Chicago to Manitou, and to Messrs. McGuiness and Myers, the representatives of the Weather Bureau at the Pike's Peak Station, for their constant kindness and frequent assistance during our stay on the Peak. To Mr. Cable, Manager of the Pike's Peak R. R., we are also indebted for many favors.

A word as to the suitability of Pike's Peak as a site for astronomical observation. When free from the disturbing effect of forest fires the sky is of a very deep blue at the zenith, and when the conditions are very favorable the blueness persists up to within a short distance of the Sun, losing, however, much of its depth of color. During the entire time of our stay the stars appeared to be little or no brighter when seen from the Peak than when seen from Manitou, 8,000 feet below. The scintillation,

even near the zenith, was always very marked, and at no time during our stay would the seeing have been even fair. In this regard our experience agrees closely with that of the Harvard Observatory party, which visited the Peak some years ago. The altitude of the summit (14,147 feet) is not greatly inferior to that of Mont Blanc (15,780 feet), and the railroad which ascends from Manitou is a great convenience. For such observations as require a blue sky rather than good seeing, Pike's Peak (when not surrounded by forest fires) would seem to offer some important practical advantages over other mountains of equal altitude. But if good seeing is essential the Peak is not to be recommended.

The Expedition to Mount Etna.

During Professor Tacchini's visit to Chicago in August, 1893, I discussed with him the problem of coronal photography, and described our unsuccessful expedition to Pike's Peak. His observations made on Mount Etna had convinced him that the Bellini Observatory (altitude 2942^{m}) would be a suitable place for the continuation of my experiments, especially as the 12-inch equatorial, mounted under an excellent dome, would serve admirably to carry the apparatus. The cordial invitation which he extended on the part of Professor Riccò and himself led to our decision to make an expedition to Mount Etna in the spring or summer of 1894.

It had been my intention to employ on Mount Etna the apparatus used at Pike's Peak, and some changes were made in it for the purpose of correcting the defects discovered in the course of our previous experiments. But during the winter Mr. Ranyard visited us in Berlin, and was kind enough to propose that I make use of a spectroheliograph to be built by Otto Toepfer, of Potsdam, for the 18-inch reflector of his London Observatory. I take this opportunity to express to Mr. Ranyard my warmest thanks for his kindness in allowing me to design the spectroheliograph as well as to employ it in the work at Mount Etna.

Experience with many forms of spectroheliograph has clearly demonstrated that the instrument should be so constructed that the line of the spectrum can be most easily kept on the second slit during the exposure. I have already pointed out* the best means of accomplishing this, and in designing the instruments used in the first experiments in coronal photography the mechanical advantages of this type were fully recognized. It was feared, how-

* *Astronomy and Astro-Physics*, March, 1893, p. 256.

PLATE XXVII.

Fig. 1.

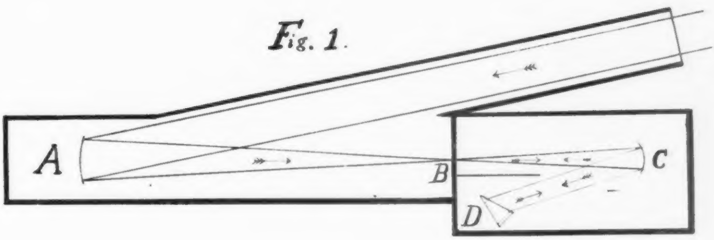


Fig. 2.

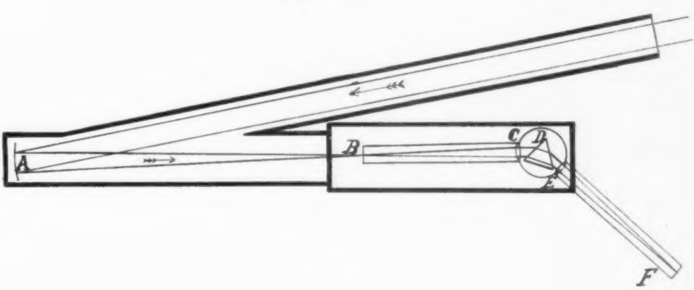
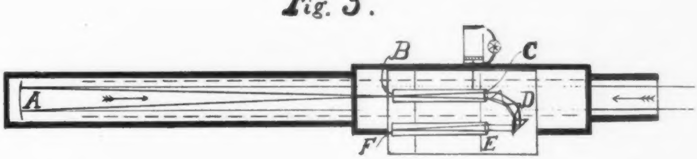


Fig. 3.

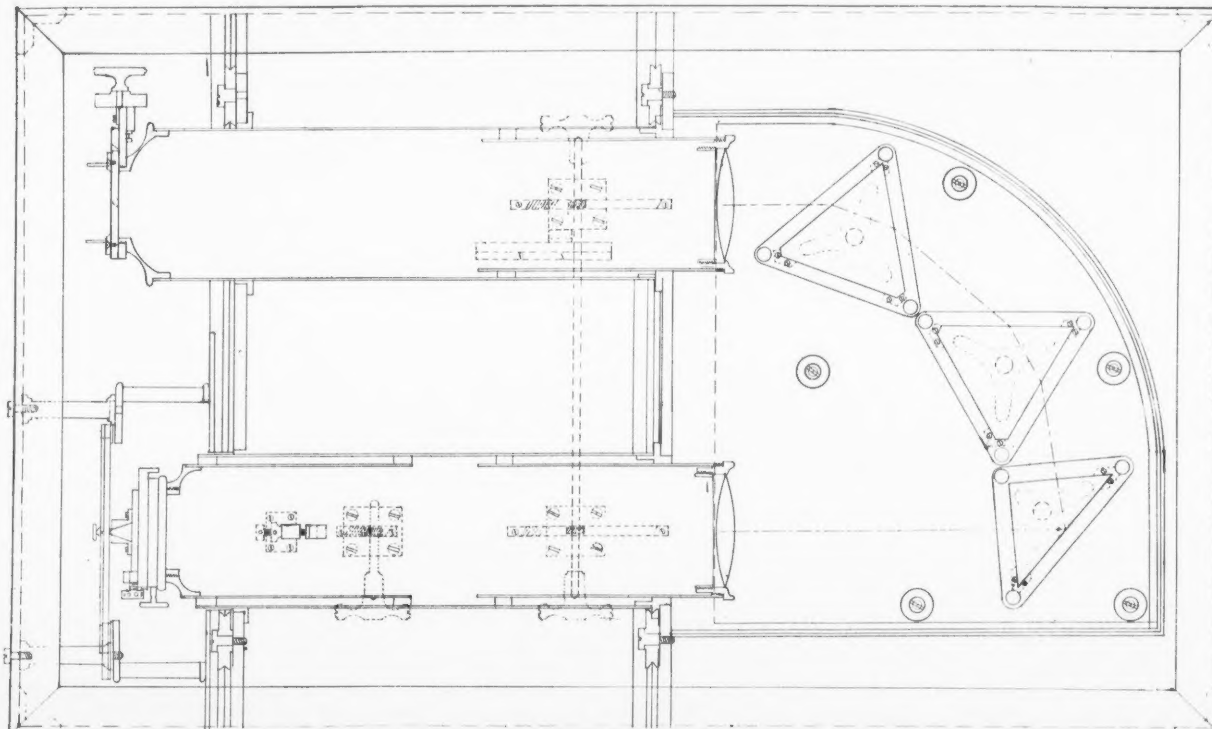


THE KENWOOD, PIKE'S PEAK AND MOUNT ETNA CORONAGRAPHS

(SCHEMATIC).

ASTRONOMY AND ASTRO-PHYSICS, No. 128.

PLATE XXVIII.



MR. A. COWPER RANYARD'S SPECTROHELIOGRAPH.

ASTRONOMY AND ASTRO-PHYSICS, No. 128.

ever, that the use of a reflecting surface in the prism train would be seriously disadvantageous in the delicate work on the corona, though not objectionable for investigations of the chromosphere and prominences. But the necessarily short focal-length of the collimator and telescope introduced such difficulties in the use of moving slits that it was finally decided that the mechanical advantages of the spectroheliograph with fixed slits would outweigh the slight objections that might be urged against it from an optical point of view. This reason, and the fact that the apparatus was ultimately intended for prominence work, decided me in favor of the design outlined in Fig. 3, Plate XXVII, and shown in more detail in Plate XXVIII.

The collimator and telescope are of $2\frac{1}{8}$ inches clear aperture and $11\frac{3}{4}$ inches focal length, and are rigidly fixed with their axes parallel. The objectives are focussed simultaneously by turning a steel rod carrying both pinions, and their position is given by a millimeter scale on the collimator tube. The frame which carries the telescopes supports also the tight metallic box containing the prism train (which is permanently fixed at the position of minimum deviation for K), and the whole can be moved in the plane of dispersion on eight grooved wheels running on four knife-edge rails attached to the top and bottom of the outside supporting steel frame. The first slit is $1\frac{3}{8}$ inches long, with one jaw fixed, and the other adjustable in width by means of a micrometer head. A narrow steel arm, carrying a blackened disk slightly exceeding the solar image in diameter, can be swung into or out of place, thus providing a means of excluding direct sunlight from the spectroheliograph. The second slit is of the same length, as the first, and has blackened jaws curved to correspond with the curvature of the K line. By means of a fine screw the slit can be moved as a whole in the focal plane, thus allowing the dark K line to be brought centrally between the jaws. The slit is then narrowed down to the proper width, both jaws being moved together toward the center by means of another micrometer screw. As the slit cannot conveniently be observed directly when the apparatus is in use, it is viewed through a microscope of low magnifying power with diagonal prism. The metallic plate-holder carries plates $2\frac{3}{8} \times 3\frac{9}{16}$ inches, and is held in a light-tight box with the sensitive surface of the plate in the focal plane. After the slide is drawn the second slit is brought almost into contact with the plate by means of a rack and pinion on the telescope tube. The objectives and prism train are of carefully selected glass, and were specially made for this apparatus by the Zeiss Optical Co.,

under the direction of Professor Abbe and Dr. Czapski, to whom I am indebted for undertaking the calculation of the curves of objectives best suited to the purpose in view. The large size of the prisms was rendered necessary by the short focal length of Mr. Ranyard's reflector. The advantages of this form of spectroheliograph are very evident in this connection, for with moving slits the prisms would have to be much larger. I should, of course, have preferred to use a collimator and telescope of twice the focal length of those here employed, but this, for obvious reasons, was out of the question. Had it been possible, one 60° prism, with the reflecting prism, would have been sufficient. With the short collimator and telescope, and the low dispersion of the light crown glass of the prisms, the K line is only about $\frac{1}{250}$ of an inch wide, and it could not advantageously be narrower. The clepsydra, which is not shown in the drawing, is similar to those employed at the Kenwood Observatory. It is attached to one side of the outer frame, and the piston rod is screwed to a projection on the side of the collimator near the objective. A cord is attached to a ring on the movable frame, and after passing around two pulleys on the fixed frame, and one hanging from the dome of the Observatory, it terminates in an iron weight. This furnishes the moving power, and the clepsydra acts merely as a regulator of the speed, the liquid (a mixture of 1 part glycerine and 20 parts water) being forced through a brass pipe from one end of the cylinder to the other. A micrometer valve in the pipe regulates the size of the opening and consequently the speed of the piston. This simple means of producing a uniform rectilinear motion is very satisfactory, as the movement is smooth, and its rapidity can be varied within wide limits.

The spectroheliograph was carried in the skeleton iron frame previously used with the Pike's Peak instrument, and the same telescope tubes and support for the mirror were used. The silver-on-glass mirror was replaced by a 4-inch mirror of speculum metal by Brashear of the same focal length (48 inches). The telescope tubes, and the collimator, telescope and prism train of the spectroheliograph were provided with a large number of diaphragms to guard against diffuse light.

Through the kindness of Dr. Rubens an attempt was made at the Physical Institute of the University of Berlin to deposit a film of platinum and gold on my glass mirror by an electrical process used with great success in silvering smaller mirrors. The advantages of such a mirror for the Mount Etna expedition

would lie in its freedom from attack by the sulphurous fumes, and in the perfect polish of the surface, for with the electrical process no hand polishing is necessary, and the mirrors are consequently wholly free from microscopic scratches. A large number of experiments were made, but it was found impossible to get a satisfactory deposit of platinum and gold on so large a surface. Of course silver could not be used, on account of the sulphurous fumes of the volcano. My thanks are due to Dr. Rubens for the numerous attempts he was kind enough to make in the hope of securing the desired deposit. It is to be hoped that future improvements of this process may render possible the production of perfect platinum and gold films. The silvered mirrors are to be highly recommended when circumstances permit of their use.

Our party, consisting of Professor Riccò, Director of the Bellini Observatories of Catania and Etna, Signorina Riccò, Antonino Capra, mechanician of the observatories, Mrs. Hale and the writer, left Catania on July 7, 1894. After a drive of three hours we arrived at Nicolosi, where we spent the night. I refer to our note-book for the following:

JULY 8. Left Nicolosi at 6 A. M. Our party had been joined by Antonio Galvagno, custodian of the Etna Observatory, and Santo Messina, assistant. Ten mules managed by three muleteers were needed to carry the members of the party and the apparatus, provisions, etc. Arrived at Casa del Bosco (1450^m) at 8^h 30^m. Examined sky frequently, and found slight decrease of whiteness as we ascended. Crossed lava stream of 1892, and had excellent view of the craters of that year, the latest of which still emits vapor. Arrived at the Observatory at 1^h 35^m. The temperature had fallen to 9° C., and the sky was nearly covered with clouds. Half an hour later we were enveloped in cloud, which surrounded us until evening, when sky was whitish, with marked halo around Moon. Stars unsteady, even in zenith.

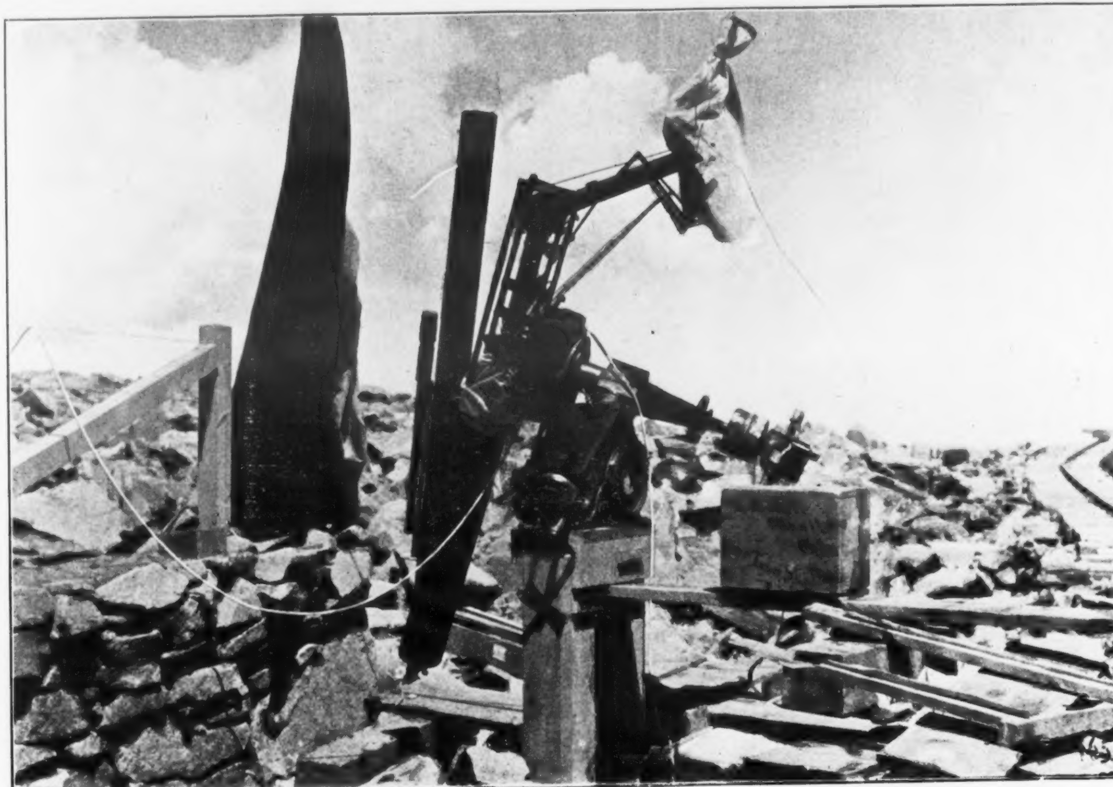
JULY 9. Sky clear, with strong wind blowing the smoke from the great crater (which rose behind the Observatory to an altitude of 3312^m) away from the direction of the Sun. Half the Island of Sicily was dimly visible from the Observatory through a great brown bank of thick haze, the upper surface of which seemed to be nearly on a level with us. Cumulus clouds commenced to form at 9^h, and soon the sky was nearly covered. At 12^h the Sun was seen between passing clouds to be surrounded with a bright halo. Unpacked and cleaned apparatus, put part of it together and made fittings for attaching it to tube of 12-inch telescope. Wind changed to west in the afternoon, and sky became much whiter.

JULY 10. Wind blew smoke of great crater over Sun, making sky very white. Observed Sun with Professor Riccò by projection with 12-inch telescope. Image rather better than at Catania, but became unsteady later. At 10^h some small cumulus clouds had formed, and Sun was surrounded by bright halo. Clouds of insects were also noticed in direction of Sun, as on Pike's Peak. Observed prominences with Professor Riccò, but images were no better than at Catania. Attached spectroheliograph to telescope tube (see Fig. 2, Plate XXX). At sunset watched shadow of Etna from the Torre del Filosofo. Whole sky covered with dense haze. At 7^h 30^m made exposures on Moon with Huggins coronagraph, but obtained nothing with 4 seconds.

JULY 11. Sky very white, bright ring around Sun. Observed atmospheric lines with direct vision spectroscope. Balanced telescope, and observed Sun by projection. Seeing excellent, granulation, spots and faculæ well defined. Strong odor of sulphur. Attached clepsydra and found motion of spectroheliograph to be very uniform. Adjusted mirror, and found effective aperture to be 3¼ inches. Adjusted spectroheliograph; definition of spectrum excellent; very little diffuse light. Made several photographs of spectrum for focus. With 3 seconds exposure all came out positives (using direct sunlight). Made an exposure for faculæ; motion perfect, and exposure uniform clear across image. At sunset visited Valle del Bove. Sky filled with haze, and almost too bright for the eye 10° from Sun. Professor Riccò made several exposures on Moon with Huggins coronagraph. Nothing shown with 4 seconds. Photographed lunar spectrum with spectroheliograph, and obtained fair result with 40 seconds exposure (Schleussner plate). If the lower corona were no brighter than the Moon in first quarter and the second slit were $\frac{1}{250}$ of an inch wide, the movement of the spectroheliograph should be 1 inch in 166 minutes. The diameter of the solar image is less than ½ inch and a motion of the slits of perhaps ¼ inch might suffice to indicate the presence of coronal structure. As the full Moon is considerably brighter than the Moon at first quarter, and as the inner corona is probably brighter than the full Moon, a motion of ½ inch in 25 minutes should suffice.

JULY 12. Sky very white. Wind still blowing smoke from crater over Sun. Bank of haze above level of Observatory. Observed Sun by projection with Professor Riccò; image unsteady. Made several photographs of spectra and facula plates with spectroheliograph. Improved adjustment of mirror for focus. Climbed to top of great crater, and found sky in zenith

PLATE XXIX.

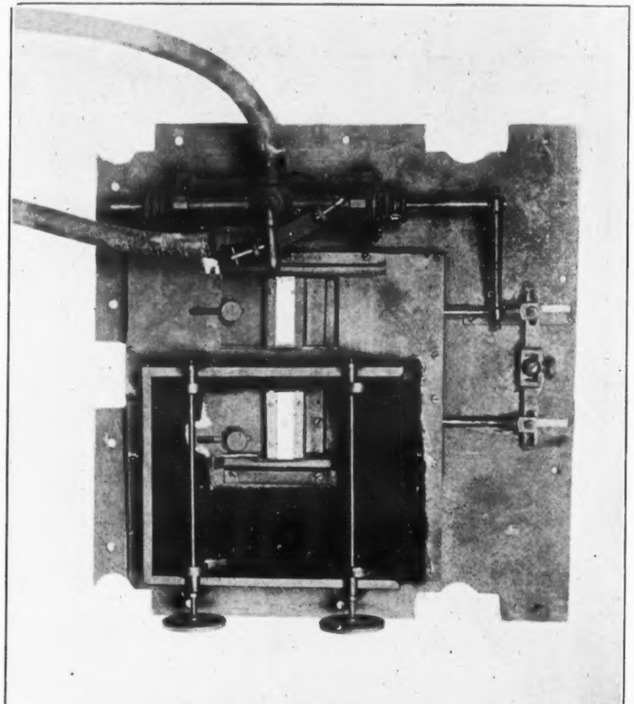


The Coronagraph on Pike's Peak.

Astronomy and Astro-Physics, No. 138.

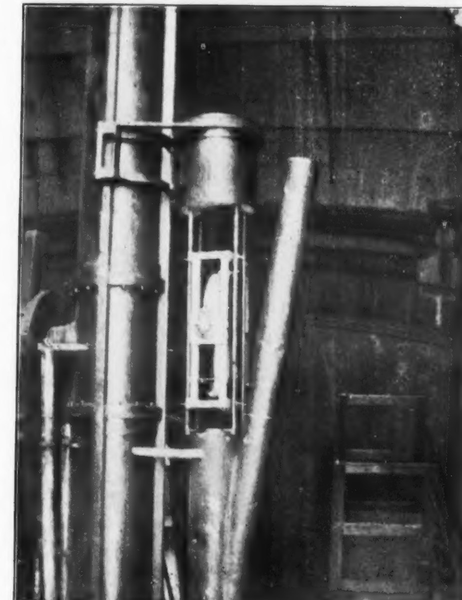
PLATE XXX.

Fig. 1.



Moving Slits of the Kenwood Observatory Coronagraph.

Fig. 2.



Coronagraph Attached to the 12-inch Equatorial of the Mount Etna Observatory.

of deeper blue than when seen from Observatory. Whole Island enveloped in haze. Descended to Observatory by moonlight; double halo around Moon. Observed Moon, Saturn and several stars with the 12-inch, using powers up to 430. Seeing *magnificent*; images almost perfectly steady with highest power. Both Moon and Saturn were very low, but images were remarkably good. With naked eye scintillation was hardly perceptible in stars higher than 30°.

JULY 13. Wind blowing from direction of crater, but sky best since July 9. Sky cloudless and generally whitish, but increase in brightness toward Sun was gradual. Much dust. Telescope in use until 9^h 40^m by Professor Riccò for daily record of chromosphere. Prominences very well seen. At 9^h 50^m broad and brilliant ring of whiteness around Sun, making it useless to try for corona. Smoke blowing directly over Sun, and diffusing through entire sky. Solar image observed by projection; definition very poor. At 11^h sky had improved, and preparations were made to photograph corona, but five minutes later more smoke blew over Sun, and sky became very white. Mirror found to be dewed, and surface badly tarnished by the sulphurous fumes, though it had been tightly covered every moment it was not in use. Sky around Sun remained bright, and wind was so violent that no photographs could be made. Strong sulphurous odor.

JULY 14. Smoke blowing across Sun. Strong sulphurous odor. Whole eastern sky white. Prominences fairly well seen at 7^h 45^m. Professor Riccò made several facula plates with spectroheliograph. Took out the tarnished mirror, but left apparatus attached to telescope, carefully wrapped up to protect it from the sulphurous vapors. Left Observatory at 3^h, and arrived at Catania about midnight.

JULY 15. Left Catania for Rome. During the journey to Messina heavy clouds came up from the west, and when we last saw the Island after leaving Reggio it was almost hidden by clouds, with Etna faintly visible through a thick haze.

Before leaving Catania I cleaned and polished the mirror as well as I could, but the result of the action of the sulphurous vapors of the volcano remained plainly visible in spite of my efforts to remove it. The apparatus had been left on the mountain, as Professor Riccò had kindly consented to attempt to photograph the corona with it in August, if the weather and sky were favorable. It had been hoped that the platinum-gold mirror mentioned above would be ready for use on this occasion, but failure to produce a perfect surface left him only the tarnished

speculum metal mirror, and good results could hardly be expected from this.

As I am assured by Professors Tacchini and Riccò that the sky is frequently very good on Etna I conclude that the difficulties we encountered were exceptional. During the entire time of our stay in southern Italy and Sicily the atmosphere was very hazy, and the sky was rarely of a deep blue. I was told by Galvagno, the custodian of the Etna Observatory, that the smoke this year has been much more noticeable than usual. If the wind had blown it away from, instead of toward us, the sky would probably have been good, though I think by no means equal to the sky seen on Pike's Peak during the first part of our stay.

I take pleasure in acknowledging our sense of deep indebtedness to Professor Tacchini, and to Professor Riccò and his assistants, for them any favors shown us during our stay in Italy.

SUGGESTIONS FOR FUTURE WORK.

While it can hardly be said that the results of my various attempts to photograph the corona without an eclipse have been at all encouraging, I have by no means abandoned hope that the method, if fairly tried under good conditions, may yet be successful. In choosing a site great care should be taken. On Pike's Peak the dust was very troublesome, and a small bellows was therefore provided to be used on Mount Etna to blow the dust from the mirror during the exposure. There was much more dust on Etna than on the Peak, and the bellows would have been useful had it been possible to carry out the proposed experiments on the corona. A snow-covered peak might offer important advantages in this and in other particulars. The season chosen should naturally be not far from the summer solstice, but the local meteorlogical records should be consulted before fixing the time. An altitude of at least 13,000 feet should be selected, and the higher one goes the better. A low latitude would naturally be preferred to a high one, on account of the altitude of the Sun.

In conclusion, I may say that the investigation has been a fascinating one, in spite of its succession of failures. On account of the importance of discovering some means of observing the corona without an eclipse, it is to be hoped that others may think it worth their while to continue the attack on this difficult problem.

KENWOOD OBSERVATORY, University of Chicago.

Sept. 7, 1894.

ADDENDUM. The following extracts are from a letter received from Professor Riccò just as the article is going to press.

CATANIA, Aug. 31, 1894.

"I did not return from the Etna Observatory until Monday (Aug. 27) as the weather continued unfavorable after your apparatus was once more ready for use. On the 24th, however, the sky was absolutely blue up to the Sun, and around it there was no halo; on the 25th it was cloudy, and on the 26th the Sun was surrounded by a faint whitish halo on a blue sky. The gold-platinum mirror did not reach me, and the other, as you know, was very considerably tarnished, and perhaps still more so after the experiments I had made in photographing the corona. I attempted to polish it, as you had done, with distilled water and alcohol applied with absorbent cotton; I carried this operation to such a point that in the shade the mirror appeared quiteclean, but in the sunlight a veil of oxide or sulphur was still visible, and I had to use the mirror in this condition.

I made about twenty photographs of the corona—seven of them on the 24th, when the sky near the Sun seemed to be perfect. I saw immediately that the necessary exposure was much less than the estimated value: 37 minutes for 11 mm. With an exposure of 37^m for the entire run of the spectroheliograph the sky around the Sun is so dark that no trace of the corona can be seen. But with exposures ranging from 1 to 10 minutes for the whole run of $0^m.05$, coronal images are obtained. As you will see in the plates which I send, the corona is plainly visible, and is better than that obtained with the Huggins apparatus, a copy of which—one of the last attempts I made on Etna when the sky was very good—I also send you. But this advantage may be only apparent, and due to the smaller size of the solar image in your apparatus. I have not been able to see details or indications of structure in the corona, and this leads me to suspect that we are still dealing with an atmospheric corona. It is, however, remarkable that while nothing was visible to the eye near the Sun, your apparatus gives a corona marked by a rift which is obscure in the light of the atmosphere!"

The photographs of which Professor Riccò speaks have not yet arrived, but the short exposures with which they were obtained seem to be insuffiicient to show the true corona. On account of the large amount of diffuse light which the tarnished mirror must have given rise to, it is not surprising that the sky around the Sun was fogged with the longer exposures.

PROFESSOR FROST'S TRANSLATION AND REVISION OF DIE SPECTRALANALYSE DER GESTIRNE.*

JAMES E. KEELER.

When Dr. Scheiner's "Spectralanalyse der Gestirne" appeared, about the end of 1890, it took its place at once as the standard treatise on astronomical spectroscopy. There was, in fact, no other book that met the requirements of the student and the specialist. The well-known treatise of Schellen, excellent of its kind and in its day, was of too popular a character for this purpose, and notwithstanding several revisions by competent authorities, it had been left far behind by the advance of knowledge, so that in 1890 it represented very imperfectly the state of a science whose growth has been so phenomenal as that of celestial spectroscopy. The value of Dr. Scheiner's book was at once recognized. It was thorough, well arranged and well balanced; it adequately represented the latest stage of astro-physical inquiry; and it was written in a clear and simple style, especially marked by the absence of the "wounded snake" sentences and intricate nests of dependent clauses which make some German works such exasperating reading for the foreigner.

All these good qualities are preserved in Professor Frost's translation, which follows the original quite closely in such portions as have not required modification in view of recent discovery. The English work is, however, very far from being a mere translation. Much new matter has been added, and some portions of the original, relating to subjects in which the advance of knowledge has been most rapid, have been largely rewritten. Notwithstanding this amount of additional matter, the size of the the book has not been increased, partly on account of the somewhat smaller type in which it is printed, but mainly owing to the fact that about one page in ten was saved in translating from German into English. (The conclusion which Professor Frost draws from this as to the relative compactness of the two languages is perhaps somewhat too general). The book is thus brought fully up to the date of publication, and since the proof was submitted to Professor Scheiner for criticism before printing, it may be regarded as a revised edition in English of the "Spec-

* *A Treatise on Astronomical Spectroscopy:* being a translation of "Die Spectralanalyse der Gestirne," by Professor Dr. J. Scheiner, Assistant at the Royal Astrophysical Observatory at Potsdam. Translated, revised and enlarged by Edwin Brant Frost, M. A., Assistant Professor of Astronomy in Dartmouth College. Ginn & Company, Boston and London, 1894.

tralanalyse der Gestirne." The treatment of all recent matter is, however, Professor Frost's. Owing to the difficulties and delays of communication, it was possible to submit only the electrotype proofs to Professor Scheiner, and in these no changes could be made. When the translator and the author differed in their views, a note was made by the author, expressing his dissent from the statements in the text, and the few pages in which these notes are collected are not the least interesting part of the book. The conservative attitude of Professor Scheiner may be noticed here, and in some cases his caution seems to be carried to an extreme. In view of the evidence accumulated by Lockyer and other observers, the measurements of Rowland, and the photographic comparisons by Kayser and Runge, most spectroscopists would regard the presence of carbon in the Sun as pretty well established. Some adverse comments on recent observations have been slightly softened down by the translator, while the brief review of Lockyer's meteoritic hypothesis in the original has been altogether omitted.

An important change, to which Professor Scheiner has given his assent, and which cannot fail to meet with general approval, is the substitution of Rowland's scale of wave-lengths for the Potsdam scale employed in the first edition. The absolute values are probably as nearly correct in one system as in the other, but for spectroscopic purposes the absolute values of wave-lengths are of far less importance than the relative ones, and while the Potsdam measurements were made with all the precision which was possible with the instruments available at the time, the large concave gratings and the photographic methods employed by Rowland give his measures a very great superiority in this respect. A further reason for preferring this system is that it has been adopted by all recent investigators of metallic spectra, and there can be no doubt that it will be exclusively used by spectroscopists in the future. At the same time a complete catalogue of the solar lines, from measurements of the original negatives, would be very useful, and it is to be hoped that such a catalogue will soon be forthcoming.

We now pass to a more detailed comparison of the translation with the original, in which, however, it is only possible to notice the more salient features. Comparatively few changes have been made in the chapters relating to spectroscopic apparatus. Schuster's method of adjusting the collimator and observing telescope is briefly described, and there is a paragraph on Lord Rayleigh's investigations on the resolving power of a spectroscope. A better

figure has been introduced to illustrate the action of the cylindrical lens. The method of studying the chromatic aberration of an objective by means of its color curve is given as in the original, but a knowledge of the color curve of a large telescope is so useful in practical work with the spectroscope that a more complete account of its determination, and of the precautions necessary to ensure accuracy in the observations, might well have found a place in a book designed for students.

In the second chapter the treatment of the concave grating is considerably expanded, and various practical details relating to its use are given. To the same chapter have been added a description of Michelson's interferential refractometer and its application to the study of spectral lines, and an account of Hale's spectroheliograph, illustrated by a plate. Another plate, from ASTRONOMY AND ASTRO-PHYSICS, shows the details of the Lick spectroscope.

Part II, on spectroscopic theories (Kirchhoff's law and Doppler's principle) has been left unchanged, except that two pages have been added on special applications of Doppler's principle, such as measurements of the Sun's rotation, the discovery of spectroscopic binaries, and motions of gases in the atmosphere of the Sun.

The changes in the chapter on the solar spectrum consist mainly in the substitution of more accurate wave-lengths for the values given in the earlier edition. The remarks by Dr. Scheiner on the unsatisfactory state of the wave-lengths of metallic lines now fortunately admit of considerable modification. Certain identification of solar with metallic lines has in many cases taken the place of conjecture, and Rowland's tables of comparison are given at length. In this connection the bearing of Kayser and Runge's researches on the identification of solar lines might have been referred to. Among the important additions is an account of Langley's work in the infra-red region of the spectrum. The recent investigations of Hale are of course explained at length, and Young's table of chromospheric lines has been considerably extended toward the ultra-violet as one of the results of these researches and those of Deslandres.

The chapter on the spectra of the planets remains unchanged, as no important observations have been made in this field since the publication of the first edition.

Extensive changes have been made in the chapters devoted to the spectra of the stars and nebulæ, corresponding to the great advances which have been made in those departments, and in

fact the matter in this portion of the book has been largely recast. Vogel's classification of stellar spectra, as modified by Scheiner, is naturally the one adopted, though Professor Frost has added the following comment (p. 238): "[This system of classification has necessarily been followed by the translator, but it is proper to state that many of the leading spectroscopists are of the opinion that the time has not yet come for an attempt at a classification along the lines of stellar development, and that any classification must for the present be regarded simply as provisional.]" These remarks, being enclosed in brackets, do not seem to meet with the unqualified approval of the author; nevertheless, the most recent, as well as the earlier investigations, justify their insertion. Only the upper portions of star spectra, where photography is applicable, have been adequately studied, and it would be easy to mention special cases in which a classification based on the appearance of this region would be in error, or at least fail to represent the entire truth. The stars of class I*b* are too few in number to be regarded as a general transition form, and no classification except Lockyer's attempts to trace the connection between Secchi's third and fourth types; while the facts brought out by the researches of Campbell on the Wolf-Rayet stars, the character of the spectra of the nuclei of planetary nebulæ, and the association of stars of class I*a* and class I*c* with extended nebulæ show that there is still much doubt as to the position which should be assigned to the various kinds of bright-line stars in a general classification based on a theory of development.

Among the more important additions to the subject of stellar spectroscopy, we naturally find full discussions of β Lyræ and Nova Aurigæ. Belopolsky's photographic observations of the former star, and the conclusions which he draws from them are given; those of Vogel at Potsdam probably appeared too late to be included in the text, and are only referred to by Professor Scheiner in a note. In view of these latter observations, Professor Scheiner considers, no doubt justly, that Belopolsky's comparatively simple hypothesis respecting the system of β Lyræ is inadequate to explain the facts. The Potsdam observations, as well as those of Lockyer, still more recently published, make it probable that the system contains more than two bodies.

An excellent abstract is given of the large mass of material obtained by observations of Nova Aurigæ, and the various hypotheses advanced to account for the observed phenomena are impartially discussed. In one of his notes on page ix, Professor

Scheiner singularly enough makes the objection to Seeliger's hypothesis that it does not at all relieve the difficulty of accounting for the high relative velocity of the bodies composing the system; whereas most persons who have followed Professor Seeliger's arguments would probably agree that his hypothesis accounts satisfactorily for the high relative velocities, but does not explain the very great absolute velocity of 600 or 700 kilometres per second which the body giving the absorption spectrum must have possessed, or the changes in its velocity indicated by the observations at Mt. Hamilton. The importance of the remarkable fact that the spectrum of Nova Normæ was almost identical with that of Nova Aurigæ, not only with regard to the bright and dark lines, but to the amount and direction of their relative displacement, has probably not yet been fully recognized. Should other new stars be found to have the same spectrum, our ideas as to the nature of these apparitions would undergo a profound change. Several plates illustrating the spectrum of Nova Aurigæ have been introduced, among them an enlargement of a photograph taken by the translator with the small spectrograph at Potsdam.

A considerable amount of new material has been added to the account of the Wolf-Rayet stars, and Campbell's measurements of the bright lines in their spectra are given in full.

The important chapter on the displacements of spectral lines has been considerably enlarged. Campbell's convenient tables for reducing spectroscopic observations of motions in the line of sight are reproduced, but the English miles have been changed into kilometres, as German geographic miles and other units have been at other places throughout the book. The photographic methods used at Potsdam, with which Professor Frost is personally familiar, are briefly but clearly described, and Vogel's results for the motions of fifty-one stars in the line of sight are given, while the detailed comparison with the Greenwich results in the original has been omitted. At the end of the chapter is an account of Keeler's observations of nebulæ with the Lick refractor, and his table showing the motions of various nebulæ in the line of sight.

In Part IV, devoted to spectroscopic tables, Rowland's table of standard wave-lengths replaces the Potsdam tables in the first edition. Abney's wave-lengths of lines in the ultra-red are given as originally printed, on Angström's scale, the factors for reduction being unknown in this part of the spectrum. The wave-lengths of iron lines in Table III are taken from Kayser and Runge's memoir, but only such lines are given as were included in

the lists of Thalén or (for the ultra-violet portion) of Cornu. About 230 stars, mainly derived from the observations of Espin and Pickering, have been added to the catalogue of stars of classes III*a* and III*b*. Table V contains a partial revision by Professor Young of his earlier list of chromospheric lines, the wave-lengths being given on Rowland's scale; and finally the bibliography of spectroscopic literature has been revised and brought up to date.

Only the more prominent features are touched upon in the above comparison, for it is impossible to specially notice the great number of small additions, liberally interspersed throughout the original matter, which greatly increase the value of the work. It must suffice to say that full justice has been done to the labors of recent observers and that the book well represents the state of celestial spectroscopy at the present time. It would be strange if a few statements in the text should not already require modification, for the advance of this department of science is so rapid that facts accumulate even while an inventory is being taken. Thus we find that the parts relating to the spectrum of the Orion nebula and its relation to the spectra of the associated stars do not represent the results of the very latest investigations, while the statement on p. 251, that the D_3 line has never been observed as a dark line in stars whose spectra show only dark lines, must now be regarded as erroneous, since it has been shown that D_3 is dark in the spectrum of Rigel.

"Astronomical Spectroscopy" is admirably adapted to the requirements of the specialist, and large portions of it cannot fail to interest the amateur and general reader. It is well printed by Messrs. Ginn & Co., to whom Professor Frost acknowledges his obligations for undertaking the publication of a book from which pecuniary profit is hardly to be expected.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

A Collection of Astronomical Photographs.*—With the exception of the excellent plates which Mr. Ranyard has published in *Knowledge* and the few

* A Selection of Photographs of Stars, Star-Clusters and Nebulæ, together with information concerning the instruments and the methods employed in the pursuit of celestial photography. By Isaac Roberts, D. Sc., F. R. S. (London, The Universal Press, 326, High Holborn, W. C.)

scattered reproductions of astronomical photographs contained in certain recent treatises, none of the remarkable photographs which of recent years have done so much to enlarge our knowledge of stars and nebulæ have been rendered accessible to the student. One's gratitude to Dr. Roberts for the beautiful series of celestial photographs he has given us in a stout volume bearing the above title is increased by the fact that he has set an example that other celestial photographers would do well to follow. We can hardly agree with the remark in the Preface that "the photographs portray portions of the starry heavens in a form at all times available for study and *identically** as they appear to an observer aided by a powerful telescope and clear sky for observing" for that the photographic method has certain inherent short-comings is generally admitted, and as a consequence of these the plates cannot represent the appearance of the sky with absolute truthfulness. But while recognizing this we must at the same time remark that Dr. Roberts' skill has reduced these difficulties to a minimum, and the resulting photographs are of great excellence, and will prove of the highest value to astronomers in their study of the regions included in the present volume.

The book opens with photographs of Dr. Roberts' Observatory and telescope, the latter being a Grubb mounting of the usual type carrying a reflector of twenty inches aperture and ninety-eight inches focal length, and a refractor of seven inches aperture, on opposite ends of a common declination axis. Notes on the photographs, and on the reduction of star positions by means of measures of distance from certain fiducial stars follow, leading up to the Introduction of the work. This contains a brief historical account of Dr. Roberts' investigations, and describes the instrumental difficulties encountered early in the course of the volume. Dr. Roberts' first intention had been to chart the stars between the north pole and the equator on about twice the scale of Argelander's *Durchmusterung*, but the decision of the Paris Congress that an international chart should be undertaken led him to confine his work to the photography of nebulæ, clusters and special fields of stars. He considers that the published photographs will be of special use in the detection of changes in the structure of nebulæ and movements among the stars, the determination of stellar variability and the relative distribution of stars in space, and the detection of new stars and the disappearance of others.

The old discussion of the relative advantages of the refractor and reflector is dismissed by Dr. Roberts with a few words. On account of its ease and permanence of adjustment he considers the refractor to be superior for the purposes of charting stars by photography, but for the work with which the present volume more especially deals—the photography of faint stars and nebulosity—he prefers the reflector, "provided it is brought under the necessary control." The reason of this preference seems to lie in the belief that the silver-on-glass reflector, used with the photographic plate in the principal focal plane, is superior in its "photo-effect" to an ordinary two-lens photographic refractor, on account of the imperfect color correction and the loss of light by reflection and absorption in the latter. The practical suggestions embodied in the remarks which follow on the requirements and adjustments of a reflector for celestial photography will be of value to other investigators in the same field of work. Especially interesting is the description of an "ideal reflector" for the photography of stars and nebulæ. It is proposed that in this instrument the guiding telescope be attached to the tube of the reflector, the arrangement generally adopted. In the description of

* The italics are ours.

the method employed in collimating the speculum of the telescope, one is led to think that a plane mirror, with its center cut out to contain the plate carrier, thus leaving a strip of mirror perhaps half an inch wide on each side of the plate, might perhaps serve well as a substitute, or at least as a useful aid, for the guiding telescope, which in any case seems out of place at the end of the declination axis, where the danger of unequal flexure is a maximum. But if the instrument meets the severe tests described in the following paragraphs it can hardly be considered to possess any serious defects. After some remarks on the purely photographic questions involved, the description of the plate of the Great Nebula in Andromeda introduces us to the principal portion of the volume. The fifty-three plates, which in all cases are from enlargements of the original negatives, are reproduced in a fairly satisfactory way, but some of them—notably the remarkable photograph of the Great Nebula in Andromeda—have been much more successfully brought out in the pages of *Knowledge*. Anyone familiar with the photo-gravure process will appreciate, however, the immense difficulties attending the publication of a work of this kind. The only criticism we feel inclined to make is that many of the photographs, notably those of star clusters, have been enlarged so much that the grain of the plate becomes unpleasantly prominent. We do not share the rather common belief that great enlargement improves astronomical photographs. A certain amount of enlargement is often necessary to bring out on prints certain small details of structure, but it should always be remembered that the enlargement can show nothing which is not on the original negative, and it is usually best not to arrive at the point where the silver grain begins to be conspicuous.

But these matters are of minor importance, and we take great pleasure in recommending this unique collection of admirable photographs to the rapidly increasing number of astronomical photographers and to all interested in stellar astronomy.

Note on the Spectrum of the Orion Nebula.—In reference to Dr. Huggins' recent note on the Spectrum of the Great Nebula in Orion, based upon my paper* on the same subject, I desire to offer some comment.

1. I think no one has questioned the *justice of the interpretation* which Dr. and Mrs. Huggins put upon their 1888 photograph; but it seems to me there is a reasonable doubt of the *justice of the photograph.*

We know the Trapezium stars have broad dark hydrogen lines. The 1888 photograph shows their spectra to be strictly continuous at the positions of the hydrogen lines.

The 1888 photograph differs from all subsequent photographs, and ascribes a strange character to the nebular spectrum; whereas the subsequent photographs apparently agree in proving that the Orion Nebula spectrum is substantially identical with all other well-known nebular spectra.

2. Dr. Huggins writes: "As Professor Campbell's remarks on the broadening of certain portions of the lines upon our plates (pp. 391-393) seem to show that he has not understood correctly the interpretation we put upon this appearance, I may say now that the view we took, and still hold is that this broadening is purely a photographic spreading on account of the greater brightness of the line at that place."

I am at a loss to know how I could have been expected so to interpret their original remarks. They have made it clear that the "abruptly different intensi-

* In ASTRONOMY AND ASTRO-PHYSICS for May, 1894.

ties of different parts" of the line are due to the different brightness of different parts of the nebula (*Proc. Roy. Soc.*, vol. 48, p. 215), as indeed one would expect. By way of explanation of the *broadening* of the lines on their 1888 negative, they said in 1889, "On one side of the [Trapezium] star-spectra, this line [λ 3724] is a little broader than on the other side; but as a similar appearance is presented by Hγ and the stronger lines of the group, it may arise from some optical or photographic cause." By way of comment on the above sentence, they said in 1890, "We now learn [from the 1890 photographs] that this difference between two parts of the lines indicates *probably a different condition of the nebula on the two sides of the star-spectra.*"* I believe the natural interpretation of this sentence is that the broadening was caused, not by "some optical or photographic cause," as suspected in 1889, but on the contrary that it was *real* and due to "probably a different condition of the nebula" on the two sides of the Trapezium. I certainly accept, most gladly, Dr. Huggins' recent interpretation.

3. In regard to "the known variation in the visible region of the principal line to the line of hydrogen at F," I believe my interpretation of the four passages quoted (ASTRONOMY AND ASTRO-PHYSICS, pp. 386-7) is perfectly just, viz.: "some small differences of relative brilliancy" were suspected, but nevertheless were doubtful.

There can be no doubt that the relative intensities of the principal line and the Hβ line vary enormously. We may say that the ratio of their intensities varies from about 4:1 to about 4:20 for different parts of the nebula.

4. The advantage in using a short camera for reducing the exposure time with stellar spectra has long been known, and my paper had nothing to do with that. I stated that the relatively short camera "applies effectively to the study of all large objects yielding bright-line spectra; comets, large nebulæ, aurora borealis, etc.," a very different matter, involving very different principles. I had not seen these principles correctly stated before; and it was my purpose to call attention to their simplicity and the great importance of utilizing them.

Mt. Hamilton, 1894. August 16. W. W. CAMPBELL.

Dr. Huggins' 1888 Photograph of the Spectrum of the Great Nebula in Orion.—During a recent visit to the Tulse Hill Observatory I had the pleasure of examining, at Dr. Huggins' request, the remarkable photograph of the Orion nebula spectrum taken in 1888. The cut in *Proc. Roy. Soc.*, v. 46. p. 60 certainly represents very closely the appearance of the negative, which is in an excellent state of preservation. The pairs of lines on either side of the very strong line at 3724 were easily seen, and most of the lines in the group between 3825 and 3900 were visible, though they were very faint. The lines at 3959, 3975, 3988 and 3998 were seen without difficulty, but the six lines between 4116 and 4167 were so faint that with the illumination used they were made out with great difficulty in the part of the spectrum due to the nebula. The increase in width and brightness of all the lines in the star spectrum was most striking, and could not have been overlooked by the most careless observer. It is probable that with suitable illumination and more time at my disposal, I could have seen all the lines with ease. I could not find the slightest trace of the missing hydrogen lines.

GEORGE E. HALE.

A New Triple Achromatic Object-Glass.—*To the Editors of Astronomy and Astro-Physics:* I shall be much obliged if you will afford me space in your widely

* The italics are mine.

read journal for a few remarks upon the new Cooke Photo-Visual Objective, of which you gave a short account in your issue for May (page 400). For that account raises two or three points which would naturally remain unsettled in the minds of your readers, in the absence of further information.

1. It is pointed out that in my original paper read before the Royal Astronomical Society, I did not state whether the lenses were to be cemented together or not, and you rightly concluded that cementing might seriously deteriorate such a lens if of moderate or large size. This is the case, and I came to the conclusion that it would be almost impossible to so perfectly cement a large lens as to show no signs of strain at the focus. Moreover, even supposing the 2d and 3d surfaces to be cemented together, yet the 4th and 5th surfaces certainly could not be cemented together, for it is *essential* to the good performance and perfect achromatism of this objective that the 4th and 5th surfaces should be separated by an interval. Your account then proceeds to say "and without it (cementing) gradual tarnishing of the interior surfaces would seem to be unavoidable; nevertheless Messrs. Cooke & Sons guarantee the permanence of objectives made on this plan."

Here Messrs. Cooke & Sons' guarantee seems to excite surprise. Now it is of course open to anybody to doubt the practical durability of the polish and transparency of the borosilicate flint glass used for this objective in spite of the best possible reasons to the contrary; yet there are many with whom the following considerations will have much weight.

Dr. Schott and Professor Abbe of Jena, as well as two other independent chemical experts, have been consulted as to the cause of a certain iridescent but perfectly transparent film which forms over a polished surface of this flint under certain circumstances. All four were unanimously of opinion that, looking to the composition of the glass (which contains boracic acid) the only impurities in atmospheric air which could tarnish it are the sulphur compounds, sulphuretted hydrogen, sulphurous acid and sulphuric acid. These impurities are well known to exist in the products of gas combustion, and it is a significant fact that a film of tarnish can be produced on this glass by a few days' open exposure in a room in which gas lights are burning, whereas if kept closed up from contact with much atmospheric impurities, no tarnish whatever ensues; pure air or even damp air have been proved to have no effect whatever upon it. Such a prolonged exposure to a damp air, as will cover a piece of ordinary hard crown glass with mould, will not touch the borosilicate flint in the least.

Now the method of mounting in its cell adopted for this new objective, renders the penetration of impure air, dampness or even dust, to the interior surface of the lenses practically impossible. To all intents and purposes the objective is hermetically sealed. I cannot enter here into an account of how this result is achieved.

2. Your account then refers to the double objective invented and patented by Professor Hastings some years ago, in which he made use of Schott's potassium silicate crown 0.13 and borosilicate flint 0.161. It is thus intimated that that flint is "nearly identical with that used by Mr. Taylor," and also that my objective "does not seem to differ materially from that proposed by Hastings."

I would like to point out that whereas the reciprocal value of the dispersion power or $\frac{\mu_D - 1}{\Delta\mu(\text{C to F})}$ of the borosilicate flint used by Professor Hastings is 46.7, the reciprocal value of the dispersion power for the new borosilicate flint used in my objective varies between 50.2 and 50.6, a very considerable difference. More-

over Dr. Schott states that the former glass, used by Professor Hastings, is no longer manufactured, for it cannot be made good enough. It cannot be asserted that this constitutes no material difference.

The U. S. patent examiners kindly sent us a short while ago (*not* in an obstructive spirit) a copy of Professor Hastings' patent specifications, which I was much interested in reading. If I remember aright, he claimed that his combination reduced the secondary spectrum to a little less than half of its present amount. This is the real amount of the reduction, and not to 5 per cent as stated in your account.

I will here give the secondary longitudinal color aberrations of, 1st, the ordinary double combination of dense silicate flint and hard crown; 2nd, Professor Hastings' combination of potassium silicate crown and dense boro-silicate flint; and, 3rd, the Cooke photo-visual objective. All are calculated for an equal focal length of 360 inches and the secondary color aberrations are calculated from the figures given in Herrn Schott's catalogue and on the supposition in all three cases that the C and F rays are united to exactly the same focus.

	Ordinary double Objective. inches	Professor Hastings' Double O. G. inches	The Cooke Photo-Visual O.G. inches
A′ (Red Potassium line)	about + .37	+ .319	− .03
C	0	0	0
D	− .12	0	+ .05
E	− .18	0?	+ .03
F	0	0	0
G	about + .8	+ .376	+ .03
H_1	about + 1.4	?	+ .09

It scarcely needs to be pointed out that Professor Hastings' combination, while achieving a possibly perfect concentration of all the rays between C and F, nevertheless falls far short of bringing the blue photographic rays to the same focus, and therefore it could not be said to be equally available for photographic purposes. I would like to remark that the figures given in the 2d and 3rd columns follow from Messrs. Schott's figures, which are after all not accurate enough to enable the foci to be calculated, in the case of the deep curved Cooke O. G., within an error of about .05 inches in so great a focal length as 360 inches. The figures in column 3 indicate that the middle part of the spectrum should fall long. However, the best test of this matter is the careful trial of a real objective of moderate or large size.

I have proved in the case of a Cooke Photo-Visual O. G. of 5 inches aperture, now almost perfected, that the central portion of the spectrum certainly does not fall beyond the focus for C and F. The most careful tests have proved conclusively that the residual secondary spectrum, and it is exceedingly minute and difficult to perceive, is of the same *tendency* as the secondary spectrum of an ordinary double objective; the brightest yellow green rays being refracted to the shortest focus, while the extreme red and extreme blue and violet rays focus very slightly long.

This I was very glad to find, because an extremely slight modification in the composition of the new melting of borosilicate flint which Messrs. Schott are now executing, or else in the composition of one or both of the other glasses, will enable me to get rid of all secondary spectrum absolutely.

In reply to your remarks upon the probable difficulty in figuring the negative lens with such a small central thickness, I will only point out that in the case of a

5-inch objective, I was anxious to see how thin it was possible to carry it, and in this case made it only .07 inch thick in the center. Nevertheless no difficulty whatever was experienced in working up its surfaces to a perfect spherical figure. I attribute the remarkable immunity from bending of this borosilicate flint to its extraordinary mechanical hardness, for rigidity and hardness go together.

H. DENNIS TAYLOR.

We are much obliged to Mr. Taylor for the information about his new objective, and must say that no fairer offer could be made than that of the Messrs. Cooke. With regard to the Hastings double objective mentioned in our May number, Mr. Taylor's recollection seems to be at fault. Although we cannot at present refer to the patent specifications, (which may possibly relate to some other form), the description of this objective in the *American Journal of Science*, vol. 37, shows that the secondary chromatic aberration is only six per cent of its amount in an objective of the usual construction, and not fifty per cent. The performance of the objective itself, which we have recently had the pleasure of examining, bears out the theoretical conclusions. The outstanding color around a bright star like Vega is hardly perceptible, and the images much resemble those formed by a reflector. We do not think however, that application to photography was contemplated in the construction of this telescope. Unfortunately the glass is hygroscopic, as stated in our former note.

The Progress of Astronomical Photography.—An address recently delivered before the section of Astronomy, Mathematics and Physics of the Australasian Association for the Advancement of Science, by the President, Mr. H. C. Russell, contains a valuable epitome of the progress of astronomical photography. Its positive statement that the corona has been photographed without an eclipse will be misleading to some, and a few other minor mistakes might be mentioned. In an article apparently aiming at some degree of completeness it is remarkable, as Miss Clerke has recently pointed out in *Knowledge*, that no mention is made of the work of Barnard, Max Wolf, von Gothard, and, we may add, Higgs. Dr. Gill's valuable investigations are barely referred to in an incidental way. But with these important exceptions the paper is a useful one, and can be recommended.

A Convenient Sensitometer.—In the *Zeitschrift fur Instrumentenkunde* for June 1894, Professor J. Scheiner, of Potsdam, describes a sensitometer of his own invention, which by its simplicity and suitability for every-day use recommends itself to those engaged in any branch of photographic work. Among the various uses to which a sensitometer can be put are mentioned the determination of the sensitiveness of plates, relation of the half tones to the strongest and weakest tones on different plates, connection between time of exposure and luminous intensity, influence of different developers on the strength of the picture, and the chemical intensity of various light sources. The apparatus consists of a circular disk, with an opening cut in it, through which the light falls upon the plate to be tested. The form of the opening is so chosen that a given distance on any part of the radius corresponds to the same intensity ratio. In front of the sensitive plate is a sheet of metal containing twenty equidistant rectangular openings corresponding in total length with the opening in the disk. Between these openings and the plate is a thin sheet of gelatine containing numbers from 1 to 20 corresponding to the rectangular openings. In order to test the sensitiveness of a plate a strip of the proper size (3×9 cm.) is cut and inserted in the carrier, and the disk is

set in rotation by a small hand wheel. Light from a special benzine lamp is then allowed to fall upon the whirling disk for one minute, and on development of the plate the highest number imprinted from the scale gives the sensitiveness of the plate on a scale of 100 by reference to a simple table. The benzine lamp is said to be very constant in brightness, so that reliable absolute determinations can be made.

A convenient form of the apparatus is supplied by Otto Toepfer, of Potsdam, and each instrument is tested by Professor Scheiner before it is sent out.

A sensitometer in all essential features identical with this was devised by Professor G. W. Hough, of the Dearborn Observatory, many years ago, and has since been employed in his photographic work. We have also used one built on Professor Hough's design at the Kenwood Observatory, and have found it very satisfactory.

Mr. Brashear's New Optical Works.—Mr. Brashear is putting up new and large buildings for his optical shops at Allegheny, Pa., the old buildings having become too small for his increasing business. The main building, of brick and steel, is 108 by 38 feet in plan, and three stories high, including a high basement. The attached boiler room is 16 by 27 feet. In the basement will be done the heavier optical work requiring a uniform temperature, and the testing of surfaces. Other optical work will be done on the floor above, a separate room being devoted to each process, and the third floor will be used as a machine shop. The building will be lighted by electricity. A more complete account of this new establishment will be given hereafter.

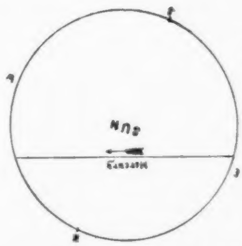
Another Great Telescope?—A paragraph has been going the rounds of the daily press, and has found its way into a number of scientific journals, to the effect that a great refractor of 50 inches aperture is to be made for an Observatory at Pittsburgh. We learn from good authority (Professor Keeler and Mr. Brashear) that the report has little foundation. An effort is being made to provide a larger telescope for the Allegheny Observatory, and to remove the Observatory to a better site, and the city has reserved sufficient ground for the latter purpose in the new park, which is well to windward of all the manufacturing establishments. No effort will, however, be made to surpass existing instruments in size; this and all further details mentioned in the paragraph seem to have been invented by an ambitious reporter on a daily newspaper.

An Object-Glass Struck by Lightning.—During a thunder-storm on Mt. Elbert in Colorado last July, the Coast Survey station on the summit was struck by lightning, and among the pieces of apparatus which were damaged was a 2½-inch Brashear object-glass belonging to a portable transit instrument. The flint lens, which in this construction is on the outside, was cracked across. A closer inspection of the outside surface shows that it is covered with many small and irregular pits, in some of which are imbedded minute pieces of metal apparently derived from the aluminium cell. The crown lens was uninjured. A new flint lens has been made for the objective by Brashear, and the old one will be kept by the Coast Survey as a curiosity.

Erratum in the August Number.—In the article by Professor Scheiner on the temperature of the stars, for "spectra which contain four lines," read "spectra which contain few lines." This misprint occurs in two places.

CURRENT CELESTIAL PHENOMENA

PLANET NOTES FOR NOVEMBER.



Mercury will be at inferior conjunction Nov. 10 at 12^h 34^m P. M. central standard time. The declinations of Sun and Mercury differ by only 4′ 53″so that the planet will be seen projected on the face of the Sun. The transit will last a little over five hours, beginning at 9^h 55^m A. M. and ending at 3^h 12^m P. M. central time. For more accurate predictions see the note on "The Transit of Mercury" on another page. The accompanying cut shows the apparent course which Mercury will take across the solar disc.

We hope that most of our readers will have the opportunity to witness this event. The best way for most to observe it will probably be by projecting the Sun's image on a white screen. Such a screen may be made of white cardboard and fastened a foot or more back of the eyepiece of the telescope by means of a wire frame. By proper focusing a very sharp image of the Sun, from six inches to a foot or more in diameter, may be obtained even with a very small telescope or spy-glass.

On the 11th at 10^h 21^m A. M. Mercury will pass by Venus, only 8′ south of the latter. On the 27th at 10^h 58^m A. M., Mercury will be at greatest elongation west from the Sun, 20° 10′. He will be at greatest brilliancy as morning planet, Nov. 26.

Venus will be at superior conjunction Nov. 30, at 9^h 17^m A. M., being then directly behind the Sun. She will not be in good position for observation during the month.

Mars has for some time been the most conspicuous object, save the Moon, in the evening sky. He far out-ranks the first magnitude stars in brilliancy, appearing almost to have a disc visible to the naked eye. Having in October passed his point of nearest approach to the Earth, he is still comparatively near and in very favorable position for observation by amateurs. He will be in conjunction with the Moon, 3° south of the latter, Nov. 9 at 12^h 56^m A. M. On the 22d he will reach the end of the westward loop in his apparent path among the stars and will then begin to move eastward.

Jupiter lights up the eastern half of the sky while Mars does the western. The two planets are nearly equal in brilliancy but quite different in color, the silvery hue of Jupiter contrasting strongly with the ruddy light of Mars. Jupiter is in good position for observation after midnight. He will be in conjunction with the Moon Nov. 16 at 4^h 04^m A. M.

Saturn and *Uranus* will be behind the Sun during November.

Neptune may be observed all night, the best time being about midnight when the planet is near the meridian. He is in Taurus not far from the star *l*.

Planet Tables for November.

MERCURY.

Date.	R. A.	Decl.	Rises.	Transits.	Sets.
	h m	° ′	h m	h m	h m
Nov. 5	15 26.4	− 20 22	7 46 A. M.	12 26.5 P. M.	5 07 P. M
15	14 43.1	− 14 10	5 03 "	11 03.9 "	5 04 "
25	14 47.6	− 13 34	5 20 "	10 29.2 "	3 38 "

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
		VENUS.			
Nov. 5	14 20.6	− 12 54	6 09 A. M.	11 20.8 A. M.	4 32 P. M.
15	15 09.9	− 16 57	6 37 "	11 30.6 "	4 25 "
25	16 01.1	− 20 16	7 04 "	11 42.6 "	4 21 "
		MARS.			
Nov. 5	1 26.6	+ 7 52	3 51 P. M.	10 25.0 P. M.	4 59 A. M
15	1 20.4	+ 7 54	3 05 "	9 39.6 "	4 14 "
25	1 19.5	+ 8 21	2 23 "	8 59.2 "	3 36 "
		JUPITER.			
Nov. 5	6 26.5	+ 23 00	7 46 P. M.	3 28.0 A. M.	11 11 A. M.
15	6 24.1	+ 23 02	7 04 "	2 46.4 "	10 29 "
25	6 19.9	+ 23 06	6 16 "	1 59.0 "	9 42 "
		SATURN.			
Nov. 5	13 54.3	− 9 19	5 28 A. M.	10 54.5 A. M.	4 21 P. M.
15	13 58.7	− 9 43	4 55 "	10 20.6 "	3 45 "
25	14 03.2	− 10 06	4 22 "	9 45.6 "	3 09 "
		URANUS.			
Nov. 5	14 52.6	− 16 11	7 07 A. M.	12 04.5 P. M.	5 02 P. M.
15	14 54.8	− 16 21	6 19 "	11 16.0 A. M.	4 13 "
25	14 57.4	− 16 32	5 44 "	10 39.6 "	3 36 "
		NEPTUNE.			
Nov. 5	4 56.4	+ 21 07	6 21 P. M.	1 54.3 A. M.	9 27 A. M.
15	4 55.4	+ 21 05	5 41 "	1 13.9 "	8 47 "
25	4 54.3	+ 21 03	5 01 "	12 33.5 "	8 06 "
		THE SUN.			
Nov. 5	14 43.6	− 15 51	6 44 A. M.	11 43.7 A. M.	4 44 P. M.
15	15 24.0	− 18 38	6 57 "	11 44.8 "	4 32 "
25	16 05.9	− 20 52	7 10 "	11 47.3 "	4 25 "

Phases and Aspects of the Moon.

		Central Time. d	h m
Apogee	Nov.	4	4 00 P. M.
First Quarter		5	9 16 A. M.
Full Moon		13	1 49 A. M.
Perigee		16	2 36 P. M.
Last Quarter		19	8 08 P. M.
New Moon		27	2 54 A. M.

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr Hartwig in the "Vierteljahrsschrift der Astronomische Geselischaft".]

MAXIMA.

Nov. 2 T Herculis.
4 R Lyræ.
5 R Ophiuchi.
5 U Geminorum.
6 S Cassiopeiæ.
8 U Capricorni.
9 T Ophiuchi.
11 S Piscium.
13 T Capricorni.
21 R Cancri.
21 U Arietis.

MAXIMA CONT.

23 R Capricorni.
26 S Orionis.

MINIMA.

Nov. 2 R Boötis.
3* R Sagittarii.
4 S Sagittarii.
8 V Tauri.
11 S Vulpeculæ.
12 R Ceti.
13 W Scorpii.
14 R Tauri.

MINIMA CONT.

15 L^2 Puppis
18 R Leonis.
19 U Cygni.
20 S Tauri.
20 V Geminorum.
23 X Libræ.
26 S Coronæ.
27 S Ursæ Majoris
27 U Monocerotis.
28 S Leonis.
29* V Boötis.
30 S Carini.
30 R Scuti.

New Variable of the Algol Type.—Dr. Krueger announces D. M. 13.3115 to be an Algol variable, discovered by Dr. Hartwig. Epoch, September 10.35 Gr. M. T.; period 2.00 days, minimum on September 16.37. JOHN RICHTIE, JR.
September 15, 1894.

* The "Vierteljahrsschrift" gives this as a maximum.

Occultations Visible at Washington.

Date 1894	Star's Name.	Magnitude.	IMMERSION Washington M. T.		IMMERSION Angle f'm N p't.	EMERSION Washington M. T.		EMERSION Angle f't N p't.	Duration.	
			h	m	°	h	m	°	h	m
Nov. 3	ω Sagittarii	5	9	11	134	9	35	179	0	24
7	h^3 Aquarii	7	10	44	85	11	43	201	0	59
7	h^4 Aquarii	8	11	42	87	12	38	204	0	56

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

Alternate Minima.

		h
Nov.	2	2 P. M.
	7	1 "
	12	1 "
	17	1 "
	22	12 M.
	27	12 "

ALGOL.

Alternate Minima.

Nov.	2	1 A. M.
	7	6 P. M.
	13	12 M.
	19	5 A. M.
	24	11 P. M.
	30	5 "

λ TAURI.

Alternate Minima.

Nov.	5	4 P. M.
	13	2 "

λ TAURI CONT.

Alternate Minima.

	h
21	11 A. M.
29	9 "

R. CANIS MAJORIS.

Every third minimum.

Nov.	3	10 A. M.
	6	8 P. M.
	10	6 A. M.
	13	3 P. M.
	17	1 A. M.
	20	11 A. M.
	23	9 P. M.
	27	7 A. M.
	30	4 P. M.

S. CANCRI.

Nov.	8	4 A. M.
	17	3 P. M.
	27	3 A. M.

S. ANTLIÆ.

Every tenth minimum.

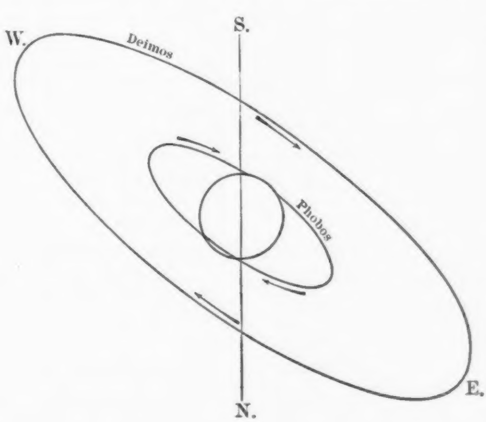
		h
Nov.	2	2 A. M.
	5	8 A. M.
	8	2 P. M.
	11	8 P. M.
	15	2 A. M.
	18	7 "
	21	1 P. M.
	24	7 P. M.
	28	1 A. M.

Y CYGNI.

Every fourth minimum.

Nov.	3	7 A. M.
	9	7 "
	15	7 "
	21	7 "
	27	7 "

The Satellites of Mars.



DEIMOS.

		h		
Nov.	1	4.5	A. M.	W.
	3	1.9	"	E.
	4	11.3	P. M.	W.
	6	8.7	"	E.
	8	6.2	"	W.
	10	3.6	"	E.
	12	1.0	"	W.
	14	10.4	A. M.	E.
	16	7.8	"	W.
	18	5.1	"	E.

PHOBOS.

Nov.	1	3.2	A. M.	W.
	2	6.0	"	E.
	3	8.8	"	W.
	4	11.6	"	E.
	5	2.4	P. M.	W.
	6	5.1	"	E.
	7	7.9	"	W.
	8	10.7	"	E.
	10	1.5	A. M.	W.
	11	4.3	"	E.
	12	7.1	"	W.

For Phobos the central time of every seventh eastern and western elongation is gven, and for. Deimos every third; the intermediate ones may be found by adding the periodic time of each satellite Periodic time of Phobos 7h 39m.2. Periodic time of Deimos 1d 6h 17m.9.

The Transit of Mercury Nov. 10, 1894.—The most important astronomical event predicted for this year is the transit of Mercury across the Sun's disc which will take place Nov. 10. This phenomenon occurs only about once in seven years, on the average, so that not many opportunities to observe it come in a life time. The amateur as well as the professional astronomer will be expected to make the most of this opportunity.

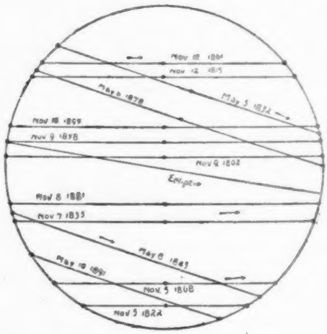


Diagram showing the Paths of Mercury acrbss the Sun's Disc during the Transits of this Century.

This transit will be visible in Western Europe and Africa, the Atlantic Ocean, North and South America, and the Pacific Ocean. The Sun will be most favorably situated for observation in Central and South America, but the conditions will also be good in the United States. The planet will enter upon the disc of the Sun near the East point, 98° around the limb from the North point, at $9^h\ 55^m\ 18^s$ A. M., Central Standard time, as seen from Northfield, and will take a northwesterly course, leaving the disc at a point 50° west from the north point, at $3^h\ 12^m\ 15^s$ P. M.

The planet will appear as a round *very black* spot, distinguishable from ordinary sunspots by its color, its roundness and its motion. The observer should watch carefully to see if, as it enters upon and leaves the disc of the Sun the planet is encircled by a ring of light, and if, when fully on the disc, it is surrounded by a narrow dusky fringe. These if seen would be evidences of an extensive atmosphere upon the planet.

ELEMENTS OF THE TRANSIT.

Greenwich mean time of conjunction in R. A. Nov. 10, $6^h\ 54^m\ 16^s.3$.

Sun and Mercury's R. A.	$15^h\ 03^m\ 44^s.68$	Hourly motion	$+10^s.12$ and $-12^s.46$.
Sun's declination	17° 18′ 58″.2S.	Hourly motion	0′ 41″.8 S
Mercury's declination	17 14 05 .2S.	Hourly motion	1 45 .2N.
Sun's equa. hor. par.	8 .94	True semidiameter	16 09 .83
Mercury's equa. hor. par.	13 .08	True semidiameter	4 .94

GREENWICH MEAN TIMES OF THE PHASES.

Ingress, exterior contact	November 10,	3^h	55^m	$31^s.2$
Ingress, interior contact		3	57	15 .4
Least distance of centres, 4′ 26″.8		6	33	48 .5
Egress, interior contact		9	10	26 .4
Egress, exterior contact		9	12	10 .4

The Greenwich mean time of exterior contacts, for any point on the Earth's sutface, may be computed from the following formulæ, in which ρ denotes the radius of the Earth at that place, φ' the geocentric north latitude and λ the longitude west from Greenwich.

$$\text{Ingress } T_1 = 3^h\ 55^m\ 31^s.2 + [0.7793]^s \rho \sin \varphi' - [1.6352]^s \rho \cos \varphi' \cos (329°\ 28'\ 15'' - \lambda)$$

$$\text{Egress } T_2 = 9^h\ 12^m\ 10^s.4 + [1.4333] \rho \sin \varphi' + [1.5339]^s \rho \cos \varphi' \cos (218°\ 51'\ 07'' - \lambda)$$

In the following table we give the Greenwich times of beginning and ending of the transit, calculated for the several Observatories from the American Nautical Almanac data:

Observatory.	Longitude.	Latitude.	Transit begins.	Transit ends.
	° ′	° ′	h m s	h m s
Harvard	71 07.7	+42 23	3 55 29 P. M.	9 12 07 P. M.
Washington	77 03.0	+38 54	3 55 24 "	9 12 06 "
Goodsell	93 09.0	+44 28	3 55 18 "	9 12 15 "
Chamberlin	104 51.9	+39 41	3 55 11 "	9 12 17 "
Lick	121 38.5	+37 20	3 55 04 "	9 12 23 "

It will be seen that the times vary only in the seconds throughout the whole United States. In order to get the Standard times we have only to subtract 5 hours for eastern, 6 hours for central, 7 hours for mountain and 8 hours for Pacific time.

Mr. Thos. Lindsay, assistant secretary of the Astronomical and Physical Society of Toronto sends us the following times of the contacts at the beginning of the transit at Northfield.

Exterior contact $9^h\ 55^m\ 50^s.0$ A. M. } Central time
Interior contact $9^h\ 57^m\ 28^s.4$ " }

These he computed from the data given in the British Nautical Almanac, considering the transit as an occultation of the Sun by Mercury.

The difference between his result and that given above is chiefly due to the difference in time of conjunction in right ascension as given in the two almanacs. The American gives the Greenwich time of conjunction as $6^h\ 54^m\ 16^s.3$ while the British almanac gives it as $6^h\ 54^m\ 47^s.6$.

Observations of the Partial Eclipse of the Moon, September 14th, 1894.—This eclipse was observed with the 12-inch and its finder and photographed with the 12-inch and the Willard lens.

The shadow seemed to be lighter in this eclipse than in others I have seen. In the 12-inch itself it was simply a pale, ashy, dusty shade, with scarcely any boundary line, though in the finder the outline of the shadow was quite marked. The limb of the Moon and details on its surface were seen while in the shadow—the limb being more conspicuous than at previous eclipses I have seen at a similar stage.

A sharp lookout was kept for the contact with the shadow. At $7^h\ 38^m.7$ I decided that the limb was certainly in the shadow. Looking in the finder it was seen that contact was certainly past, as the shadow at this time had made quite an advance on the limb.

The following observations were obtained and will be quite accurate as they were made with the finder and the outline of the shadow was well marked.

Contact with the first portion of Sinus Iridum, $7^h\ 51^m\ 50^s$.

Bisecting Plato, $7^h\ 52^m\ 40^s$.

Bisecting Plato, $7^h\ 01^m\ 10'$.

The last contact was observed at $9^h\ 25^m.7$, though a decided shade was present at that point of the limb for several minutes later. The actual contact was, I have no doubt, later than this.

Five exposures were made with the 12-inch with aperture reduced to 4 inches. These are fairly satisfactory considering the subject. Nine exposures were made with the Willard lens on three plates, a Carbutt transparency, an Eastman transparency and a Cramer "crown" plate (very rapid plate). These are satisfactory and show the phase far better than the 12-inch—just, indeed, as the naked eye shows the outline of the shadow better than the telescope does.

In this eclipse the shadow was a dark, cold gray (in finder) with no suggestion of warmth at any stage. I think it was lighter than usual.

The outline of the shadow at contacts was too indefinite to make even an approximate guess of contact times of any value—especially with such a large instrument as the 12-inch.

The recorded times are Standard Pacific—$8^h\ 0^m\ 0^s$ slow of Greenwich.

E. E. BARNARD.

Mt. Hamilton, Sept 15, 1894.

COMET NOTES.

Correction to the Photograph of Gale's Comet in Astronomy and Astro-Physics for August.—In making the half tone from the glass contact positive I sent of the photograph of Gale's Comet on May 5th, the positive has unfortunately got reversed, as will be seen by comparison with that of May 3 in ASTRONOMY AND ASTRO-PHYSICS for June.

As this may cause confusion it will be well to correct it.

The one for May 3 is correctly printed—below is west, to the right is north. The engraver having used the wrong side of the plate in that of May 5, the north and south are interchanged. In this picture the west is at the bottom but north is to the left. This is to be regretted and the orientation should be corrected on the prints.

E. E. BARNARD.

New Elements of Comet Tempel II, 1894, III (1873, II.)—In *Astronomische Nachrichten*, No. 3246, Mr. Schulhof gives new elements of this periodic comet, corrected by means of the early observations made this year. He says that it is desirable that the comet be observed as long as possible by observers having the use of powerful telescopes.

ELEMENTS.

Epoch, 1894, June 4.0, Paris M. T.

$$\left.\begin{array}{rl} M = & 7^\circ\ 53'\ 09''.5 \\ \pi = & 306\ \ 15\ \ 00\ .3 \\ \Omega = & 121\ \ 10\ \ 05\ .5 \\ i = & 12\ \ 44\ \ 21\ .9 \end{array}\right\}1894.0$$

$$\begin{array}{rl} \varphi = & 33\ \ 26\ \ 27\ .4 \\ \mu = & 679''.9391 \\ \log a = & 0.478358 \end{array}$$

NEWS AND NOTES.

We believe our readers will pardon the delay of this number when they see its contents. The cheif cause of it was the fact that the copy for some plates came to hand late. We do not blame anyone for there was good reason for the delay although the time left to us was too short for prompt publication. Will contributors please bear in mind that *all* matter should be in hand before the 15th of the month preding the one for which the publication is dated.

This is manifestly a Mars' number, and the articles and colored plates contributed to it show earnest work that already compares very well with that of the last favorable opposition. We will soon give more articles on the study of Mars, now going on in this country, which we believe will still add to the interest of our readers from the news we have already received from prominent observers who have not yet reported their observations.

Reorganization of the U. S. Naval Observatory.—On Sept. 21, H. A. Herbert, Secretary of the Navy, wrote to Professor William Harkness, saying substantially that he had appointed him Astronomical Director of the Naval Observatory, to be in charge of, and responsible for the direction, scope, quantity and preparation for publication of all work purely astronomical to be performed at the Observatory. The Secretary admits that the criticisms of astronomers in regard to the lack of system in astronomical work at the Observatory has some foundation, but that all other charges are groundless.

The new regulations under which this appointment is made provide that the Observatory, under the control of the Secretary of the Navy, is subject to the direct supervision of the bureau of equipment, that a naval officer is assigned as superintendent and that the Observatory work is divided in two branches, astronomical and nautical. The first of these includes the department of astronomical observations, and the department of the nautical almanac. The second includes the departments of nautical instruments, of chronometers, and time service and of magnetism and meteorology. The superintendent, as commanding officer, is charged with the general superintendence and government of the Observatory.

Duties of the Astronomical Director.—All officers, assistant astronomers, computers and employes are subject to his orders and he reports the operations of the Observatory annually. The astronomical director has charge of and is responsible for the direction, scope, character, quantity and preparation for publication of all work purely astronomical which is performed at the Observatory. He has charge of the 26-inch and 12-inch equatorial telescopes, the 6-inch and 9-inch transit circles and in fact all the astronomical instruments. He is to personally inspect, both day and night the methods of observation and is to present to the department on the last days of June and December of each year reports upon the qualities of his subordinates, their aptitude, efficiency, zeal, punctuality, health, and deportment.

He is not to enter into any agreement with other Observatories for the performance of work which will occupy the time of any observer more than one month without the sanction of the department.

The director of the Nautical Almanac is held directly responsible for all that pertains to that publication.

Nautical Instruments and Time.—The head of the department of nautical instruments is to see that all the instruments issued except chronometers are thoroughly inspected, that a record shall be kept of each instrument, and is held responsible for the safe keeping of instruments.

The head of the chronometer and time service department is to make all the determinations of local time that are necessary, will transmit the daily time signals and care for all the time and chronometer work of the Observatory. The head of the magnetism and meteorology department is to keep up a continuous series of observations and report any unusual disturbances, their probable cause and relation to visible phenomena.

An annual report of the operations in the various departments for the preceding fiscal year should be submitted to the superintendent July 15.

The regulations for the supervision of the library, the buildings and grounds, those governing the instrument maker, electrician, watchmen, fire organization and laborers are all placed under the control of the superintendent.

Nautical Almanac Error in Diagram of Mars' Satellite Orbits.—Professor William H. Pickering, Lowell Observatory, Flagstaff, Arizona, calls our attention to a slight error in the American Ephemeris regarding the orbits of the satellites of

Mars as found on page 456 of the Ephemeris for 1894. The arrows should be turned in the opposite direction and the orbit of Phobos should pass in front of the bottom, instead of in front of the top of the disc. This error was detected by Mr. A. E. Douglass of Lowell Observatory, September 14th, who noticed what he took to be two faint stars near the planet. He soon saw that they were moving, but not being at elongation they did not agree in position with computed places of the satellites. A little investigation soon showed where the error lay. He easily followed Phobos to within less than a radius of the planet. A glance at the diagram in the Ephemeris will show the mistake, for, since the satellites revolve nearly in the plane of the planet's equator, and the south pole is turned towards us, the southern side of the orbit of Phobos must clearly pass on the further side of the planet's south pole. Since the motion of the satellite is direct the arrows must certainly point in the opposite direction.

We had not noticed this error. We have had a new cut made showing the orbits of the satellites correctly in these two particulars. It will be found elsewhere in this number.

The Chicago Academy of Sciences.—*Section of Astronomy and Mathematics, Sept. 4th.*—The regular monthly meeting was held in the Commerce Club, Auditorium Building, Sept. 4th; Professor G. W. Hough, president, in the chair.

Professor Geo. E. Hale, who has just returned from an extensive trip abroad, read the first paper of the evening on "*Some European Observatories and the Work they are doing.*" Professor Hale gave an interesting and rather full account of his visit to the various Astro-Physical Observatories in England, France, Germany, Austria and Italy. The speaker seemed much pleased with his visit to Mr. Higgs in Liverpool, Dr. Huggins and Mr. Ranyard in London; Professor Vogel in Potsdam and Professor Tacchini in Rome. He detailed the scientific and social events attending his visit to the different observatories, and spoke in terms of warm appreciation of the courtesies and hospitalities shown him by all the European astro-physicists.

During his stay in Berlin, he had been engaged upon some researches at the Physical Institute on the effect of ultra-violet light in discharging a sodium plate in vacuo. He was greatly interested in the work he found in progress at the Potsdam Observatory, and said that Professor Vogel had shown a deep interest in the plans of the Yerkes Observatory, which were then being revised and perfected. From Berlin Professor Hale went to Vienna, and after visiting the Imperial and von Kuffner Observatories continued his journey to Rome, where several weeks were spent with Professor Tacchini at the Roman College Observatory.

The speaker then gave an account of his trip to Mt. Ætna with Professor Riccò, undertaken with the hope of photographing the corona. The experiments were unsuccessful owing to the smoke of the volcano, and the haze of the atmosphere, and he left the apparatus in charge of Professor Riccò, and returned via Paris and London.

The speaker seemed well pleased with his interesting journey, and said that many new lines of research had been suggested by the work he found in progress abroad.

In conclusion, Professor Hale thought that while the European countries are generally in advance of America in most branches of pure science, this was not true in Astro-Physics, as the great appliances of the Americans had given them the lead. The paper was illustrated by lantern projections and was highly appreciated by the members of the Academy.

Dr. See presented the second paper on "*The Locus of the Centre of Gravity for a Homogeneous Ellipsoid of Revolution,*" an investigation to which he had been led by his lectures at the University of Chicago on the Attractions and Figures of the Heavenly Bodies. The work is supplementary to the investigations of Herschel and Airy, and seemed to throw a very clear light upon the theory of the attraction of a homogeneous planet. On motion of Professor Hale it was voted to incorporate the section of Physics, so that this section of the Academy of Sciences will hereafter be known as the Section of Mathematics, Astronomy and Physics.

After some discussion the meeting adjourned. G